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Three Essays on Vulnerable Workers

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in
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by

Sarah H. Bana

Committee in charge:

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June 2019

The Dissertation of Sarah H. Bana is approved.

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Three Essays on Vulnerable Workers

To my parents, Habibullah and Khairunissa Bana.
Their love has consistently fueled my success.

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Abstract

Three Essays on Vulnerable Workers

by

Sarah H. Bana

Vulnerable workers, workers who have recently experienced a shock that could adversely affect their labor market prospects, experience large, long-lasting earnings losses – on average. This dissertation investigates the mechanisms behind the losses of three groups of vulnerable workers and the role of public policy in mitigating these losses. In the first essay, I identify which displaced workers, workers who lose their job as a result of a firm or plant closing, are the most vulnerable. I find that a worker's duration of joblessness depends much more on conditions within that worker's occupation than conditions within that worker's industry. This suggests a worker's vulnerability is a function of their skills and less related to the goods and services they were previously producing. In the second essay, my collaborators and I estimate the causal impacts of benefits in California's Paid Family Leave program on a second group of vulnerable workers: new mothers. We find no evidence that a higher weekly benefit amount increases leave duration or leads to adverse future labor market outcomes for mothers with earnings near the maximum benefit threshold. In the third essay, my collaborators and I find strong evidence that Disability Insurance and Paid Family Leave program take-up is substantially higher in firms with high earnings premiums. Our results suggest that changes in firm behavior have the potential to impact social insurance use and thus reduce an important dimension of inequality in America.

Contents

Chapter 1

Introduction

This dissertation contains three essays on vulnerable workers, workers who have recently experienced a shock that could adversely affect their labor market prospects. Each chapter explores the mechanisms behind vulnerable workers' earnings losses and the role of public policy in mitigating these losses. I identify important factors in workers' labor market success, shedding light on the earnings determination process. With a better understanding of relevant factors, I assess whether state programs are allocating resources to the most vulnerable workers.

In the first essay, I study displaced workers, workers who lose their job as a result of a firm or plant closing. On average, displaced workers experience large, long-lasting earnings losses, but some displaced workers experience larger earnings changes after displacement than others. I use comprehensive occupational employment data to estimate the effect of the state-level occupation growth rate in the worker's pre-displacement occupation on subsequent labor market outcomes. I find that adverse labor market conditions in a worker's occupation at the time of displacement have negative consequences. Displacement from a shrinking occupation is associated with decreased earnings and longer durations of joblessness. Furthermore, holding the occupation growth rate constant, there is only a small effect of the worker's industry growth rate on their labor market outcomes. These results suggest that vulnerable displaced workers' difficulties in the labor market are a function of their skills and less related to the goods and services they were previously producing. The workers at greatest risk have occupation specific human capital that is less valuable after their job loss, leading to either longer durations of joblessness or larger earnings losses.

Displaced workers are not the only workers who experience sizable and persistent earnings losses. More recently, researchers have found a similar profile of losses amongst mothers after the birth of their first child. It appears job displacement is not the only major life event with labor market consequences. The second essay investigates the effect of additional benefits on mothers who have new family responsibilities in California's Paid Family Leave program.

Specifically, with my co-authors Kelly Bedard and Maya Rossin-Slater, I use ten years of California administrative data with a regression kink design to estimate the causal impacts of benefits in the first state-level paid family leave program for women with earnings near the maximum benefit threshold. We find no evidence that a higher weekly benefit amount (WBA) increases leave duration or leads to adverse future labor market outcomes for this group. In contrast, we document that a rise in the WBA leads to an increased likelihood of returning to the pre-leave firm (conditional on any employment) and of making a subsequent paid family leave claim.

The Paid Family Leave (PFL) program in California falls under the larger umbrella of State Disability Insurance. PFL and Disability Insurance (DI) have become important sources of social insurance, with benefit payments now exceeding those of the state's Unemployment Insurance program. However, there is considerable inequality in program take-up. While existing research shows that firm-specific factors explain a significant part of the growing earnings inequality in the U.S., little is known about the role of firms in determining the use of public leave-taking benefits.

In the third essay, using administrative data from California with my co-authors Kelly Bedard, Maya Rossin-Slater, and Jenna Stearns, I find strong evidence that DI and PFL program take-up is substantially higher in firms with high earnings premiums. A one standard deviation increase in the firm premium is associated with a 57 percent higher claim rate incidence. Put differently, take-up of temporary social insurance programs is lower in lower earnings premium firms. Workers at these firms, therefore, are more vulnerable from both an earnings perspective and a benefits perspective. Our results suggest that changes in firm behavior have the potential to impact social insurance use and thus reduce an important dimension of inequality in America. Despite near-universal program eligibility for workers, non-policy-driven determinants of take-up play a major role.

1.1 Permissions and Attributions

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Chapter 2

Identifying Vulnerable Displaced Workers: The Role of State-Level Occupation Conditions

2.1 Introduction

Displaced workers, workers who lose their job as a result of a firm or plant closing, have large earnings losses on average. However, these large average losses mask substantial variation across workers. What explains this variation? Prior research shows that workers displaced when the national unemployment rate is high experience larger earnings losses than those displaced when the national unemployment rate is low. But the national unemployment rate may mask substantial differences between workers in their labor market prospects. Specifically, a worker may have more or less difficulty finding work depending on conditions in their occupation, defined as the set of activities or tasks they are paid to perform, or their industry, defined as the primary business activity of their establishment. The roles of these pre-displacement employment attributes may shed light on the circumstances under which a worker's human capital may be less valuable. This distinction is also important to effectively target job search assistance to recently unemployed workers.

Attempts to perform such an analysis have been constrained by data limitations. Specifically, because occupation is a worker-level characteristic with many options, annual occupational employment estimates to measure short-term employment fluctuations do not exist in the United States. I address this limitation by constructing a novel measure of occupation conditions that captures short-term state-level fluctuations in occupational employment by combining existing datasets on the share of each occupation in an industry and industry growth rates.

I then use data from the Current Population Survey Displaced Worker Supplement to study the effects of poor state labor market conditions in a displaced worker's occupation of origin on a number of labor market outcomes. In models comparing workers displaced from different occupations in the same state and year net of occupation fixed effects, those displaced from shrinking occupations suffer significantly longer durations of joblessness and lower earnings, conditional on being re-employed. A one standard deviation decrease in the worker's occupation growth rate (which is approximately four percentage points) is associated with a 16.1 percent increase in the duration of joblessness and a 9.2 percent decrease in weekly earnings. Additionally, I find that state-level occupation growth impacts durations of joblessness significantly more than state-level industry growth does. The estimated effect of the industry growth rate also diminishes in all models including the occupation growth rate. This supports the claim that employment prospects depend much more on workers' occupation (the set of activities or tasks that employees are paid to perform) than their industry (the primary business activity of their establishment).

The idea that state-level occupation conditions matter is quite intuitive, but their importance has not been measured due to data limitations. Unlike industry codes, which employers report when submitting information for unemployment insurance, regularly produced comprehensive occupational employment data are only available from the Bureau of Labor Statistics (BLS) Occupational Employment Statistics program, and suffers from a significant limitation. The data used to produce occupation employment estimates for each year are collected in a three year sampling cycle, which means independent annual occupation employment estimates are not produced. As a result, existing estimates cannot capture short-term fluctuations in occupational employment. I address this limitation by constructing an occupation growth rate measure using a shift-share method based on states' different occupation and industry compositions and national industry growth rates. This measure of the occupation growth rate takes into account the growth of all industries that employ workers in a particular occupation in the state to assess potential employment opportunities within a displaced worker's occupation.

To the best of my knowledge, this is the first study to create a measure of local conditions within an occupation and to estimate its importance for displaced workers' labor market outcomes. This new evidence that the relevant employment conditions are at the occupation level suggests a significant role for occupation-specific human capital relative to industry-specific human capital. In contrast to workers displaced from shrinking industries, there appears to be considerably less scope for workers from shrinking occupations to find work with similar earnings.

This research builds on literature on specific human capital, which shows that displaced workers who change occupations, or skill portfolios, lose more than displaced workers who change industries (Kambourov and Manovskii, 2009; Poletaev and Robinson, 2008). However, the decision to change occupations or industries is endogenous, making it difficult to attach a causal interpretation to these differences. By identifying the occupation growth rate, an observable factor associated with costly switching, I demonstrate a clear relationship between decreased demand for occupational services and its labor market consequences.

In addition, because industry- and occupation-switching are outcomes of the post-displacement job search process, the act of switching cannot be used to target re-employment assistance to displaced workers. In this way, this paper contributes to the literature on targeting workers who are likely to experience longer unemployment durations or large earnings losses, while speaking to the efficacy of certain re-employment policies in the United States. For example, this paper suggests that policies targeted at declining industries are poorly focused because displaced workers' difficulties are more related to their skills than the goods and services they were producing. The findings are consistent with Ebenstein et al. (2014), who find that occupational exposure to globalization is associated with significant wage effects, while industry exposure has no impact. The shocks examined in this paper apply to a broader measure of employment conditions and thus the occupations affected are likely a different, and potentially more representative, sample of workers than those affected by offshoring.

The effect of the occupation growth rate on displaced workers' labor market outcomes in this paper complements existing research on the effects of adverse labor market conditions on various groups, including displaced workers Davis and von Wachter (2011), economists (Oyer, 2006) and college graduates (Oreopoulos et al., 2012; Altonji et al., 2016). In fact, the magnitude of the main estimate in this paper (a 9.2 percent decrease in weekly earnings per standard deviation decrease in occupation growth rate) is similar to the short-run effects of graduating during a typical recession found in Oreopoulos et al. (2012) and Altonji et al. (2016). As this effect is strongest for the contemporaneous occupation growth rate and not the occupation growth rate in the prior year or two years ago, it appears that this loss can be attributed to *temporary* adverse labor market conditions. That said, unlike economy-wide recessions, the types of shocks examined here depend also on workers' state of residence and occupation. They are also net of controls for year of displacement, state of residence, and minor occupation group, and therefore demonstrate the impact of conditions even more localized to the worker. While the occupation growth rate can decline during a recession, a full-blown recession is not necessary. Instead, declines in occupational employment that come from declines in the industries where the occupation is concentrated affect labor market outcomes net of year fixed effects. As workers' employment prospects are dependent on conditions at the state and occupation level, aggregate indicators like the national unemployment rate mask the heterogeneity in employment prospects within occupations, across states, and over time.

Finally, this paper contributes to a long line of literature interested in understanding displaced workers' labor market outcomes. It relates most closely to Carrington (1993), who argued that the wage losses of high tenure displaced workers can be attributed to downturns in industry, occupation, and state labor market conditions. The major insight of Carrington's paper, echoed by Neal (1995), is that workers displaced from declining industries experienced significantly greater wage losses than workers displaced from growing industries. Based on the data available at the time, the Carrington (1993) study uses only ten occupation categories, admitting that this grouping is coarse, while the industry employment measures are finer. As a result of these data limitations, relevant employment growth at the industry level was much better measured than relevant employment growth at the occupation level, which suggested a strong role of industry conditions and, potentially, industry-specific human capital.

With better data and a new method to identify an occupation growth rate, I find that occupation growth has a significantly larger role than industry growth in determining durations of joblessness, and has a significant relationship with earnings changes, holding constant the industry growth rate. Importantly, even though my occupation growth rate is constructed, in part, from national industry growth rates, my estimates of its effects are robust to a variety of controls for industry growth, and are more important determinants of displaced workers' outcomes than industry growth in all specifications. Thus, while industry growth rates matter (consistent with previous research), my results show that industry growth rate matters mostly because it changes the mix of occupations demanded in state labor markets. Consequently, predicting the local occupation growth rate from national industry growth yields a more powerful predictor of displaced workers' outcomes than either national or local industry growth measures on their own.

2.2 Data

My dataset of individual-level outcomes comes from the Current Population Survey (CPS) Displaced Workers Survey (DWS). I link the displaced worker's pre-displacement occupation to state-level occupation conditions, created using the Occupational Employment Statistics and the Quarterly Census of Employment and Wages.

2.2.1 Displaced Workers Data

The Displaced Workers Survey is a CPS supplement administered biennially. Respondents to the CPS were asked if in the past three years, they lost or left a job because their plant or company closed or moved, their position or shift was abolished, there was insufficient work or another similar reason.[1] I use the survey years from 2004 to 2014, so workers surveyed were displaced between 2001 and 2013.

I limit my sample to individuals displaced because their plant or company closed down or moved as a plant or company closure may be less likely to spare high quality workers than mass layoffs (Gibbons and Katz, 1991).[2] Following Neal (1995), I also exclude workers reporting less than $40 of pre-displacement weekly earnings. I also limit

[1] Workers who left a job anticipating a mass layoff should respond affirmatively to this question.
[2] There is some research suggesting this may be a function of firm size (Krashinsky, 2002).

my sample to workers who have not moved since displacement. This is because the data does not specify the state in which the worker was displaced, and therefore, it is not possible to connect workers to the appropriate state-level occupation growth rate for workers who have moved since displacement.[3] The main analysis sample consists of workers who have been displaced from a full-time job. The descriptive statistics for the main analysis sample are reported in Table 2.1.

Displaced workers come from all education categories and races. The average age in the sample is 42.82 years, with 6.75 years of firm tenure. The sample is 41.9 percent female. The mean weekly earnings loss after displacement was $108.1 or 25.3 percent for workers who had been re-employed. This is in the range of previous research on displaced workers and is consistent with the unusually poor labor market conditions following the Great Recession. 70.4 percent of workers worked for pay since displacement.

Additionally, I exclude workers who do not report key variables including pre-displacement occupation, year displaced, full-time status at pre-displacement job and whether the worker moved after displacement.[4] The first two variables are necessary to create the occupation growth rate, which is the focus of my analysis, and the other two variables are key sample selection criteria.[5] This is not a trivial restriction: 15.9 percent of the respondents do not respond to these questions. In Table 2.2, I report tests for differences in observable characteristics between individuals reporting and not reporting pre-displacement occupations. Those reporting, as described, are more educated and have longer pre-displacement tenures.

[3] I show my results are very similar when including all workers - both those who have moved and those who have not moved in the Online Appendix.

[4] Very few survey respondents seem to be selectively responding to certain questions. Instead, the respondents appear to stop answering questions altogether.

[5] Occupation growth rates can also not be calculated for workers with sufficiently vague occupations – if an engineer is not one of the 17 types of engineers listed in the Standard Occupation Classification, he/she falls into the “Engineers, All Other” category, for which the data necessary for a growth rate does not exist. These workers are also excluded from the analysis.

For confidentiality reasons, the DWS does not report finer geography than state for some non-trivial fraction of the sample (approximately 30 percent). As such, state is the geographic labor market used for the analysis.

2.2.2 Occupation-Industry Composition from the Occupational Employment Statistics

To estimate the effect of the occupation growth rate, I need an annual measure at the state or local level. The American Community Survey (ACS) and other commonly used micro-data cannot be used to calculate a growth rate for most detailed occupations at the state level, since their sample size is inadequate to calculate reliable growth rates for many smaller occupations. Ideally, I would create occupation growth rates using an administrative dataset where employers reported employment levels by occupation annually.

Unfortunately, such dataset does not exist in the United States. The alternative data source for occupation level data is the Occupational Employment Statistics (OES). The OES is a large employer survey conducted by the Bureau of Labor Statistics (BLS) that collects detailed information on employment by occupation, covering 1.2 million establishments and 57 percent of employment in the United States. With a much larger sample size, it is designed to produce detailed estimates of occupation level employment and wages, though these estimates are not suitable for the study of short- term changes. The survey design selected by the BLS divides the establishments surveyed for each set of estimates into panels spread across three years of data. That is, the samples for two adjacent years are not independently drawn, and therefore cannot be used to create an annual growth rate.[6] The OES estimates reported by the BLS for a given year are moving

[6]For example, even a very large private employer will be surveyed every three years. This can make an occupational estimate produced using consecutive years of survey data very different, especially at

averages based on three years of survey data.

Even if adjacent years of data were independently drawn, estimates of a single year have greater sampling error, which may be problematic when studying detailed occupations. In fact, Abraham and Spletzer (2009) use the confidential microdata at the detailed occupation level to assess the suitability of the OES for studying the effects of offshoring. They conclude that "employment time series for detailed occupations that are created from single-year micro data are likely to be highly volatile... Increases in the size of the OES sample would be needed to reduce the variance of annual employment estimates" (p. 11).

Because of these limitations, the lack of independence across adjacent years in the sample, and the sampling error associated with a single year's estimates, the OES cannot be used by itself to produce a state-level occupation growth rate.

The OES also produces estimates of occupation by industry employment at the national level for all years. I use the estimate of occupation by industry employment in 2002 and 2003 to construct an alternative occupation growth rate, along with the industry employment numbers discussed in the next subsection.[7] The OES also produces research estimates of occupation by industry employment at the state level for 2012-2014, which I will use for robustness checks.

2.2.3 Industry Growth Rates from the Quarterly Census of Employment and Wages

The Quarterly Census of Employment and Wages (QCEW) is a tabulation of employment of all establishments that report to the Unemployment Insurance programs in the United

the local level.

[7]The use of weights at the beginning is accordance with common practice. However, the results are not sensitive to weights, for example, based on the middle of the sample.

States. This employment covers 97% of all wage and salary civilian employment in the U.S. Because every establishment is assigned to an industry, these data are reported at the industry level. I use the annual version of this dataset as the DWS respondents only report their year of displacement. Annual state-level industry employment is used in tandem with the occupation by industry employment composition to construct an estimate of changes in occupational employment. Annual state-level industry employment is also used independently to create a measure of industry growth rate. Occupation data is not available in the QCEW.

2.3 Empirical Approach

My goal is to estimate the effect of the state-level growth rate of a displaced workers' pre-displacement occupation on his or her labor market outcomes. However, as described earlier, a key challenge is that the OES occupation counts for a single year are estimated using the prior three years of data. Consequentially, major issues – lack of independence across adjacent years, and sampling error associated with a single year's estimates – impede the estimation of an unbiased coefficient.

To overcome these obstacles, I predict occupation growth from the higher quality data that are available for industry growth. In contrast to occupation level employment, industry level employment is well-measured on a yearly basis. This is because a firm's product or service determines its industry and this information is easily aggregated using administrative data from unemployment insurance records. Occupations are distributed in different proportions across industries because the composition of labor inputs varies across the production of different goods and services.

If the relevant occupation conditions are at the state level, then the occupation growth rate can be predicted using a state's industry employment composition, the state-level occupation-industry distribution, and the growth rate of the industries within the state. In the following subsection, I explain the construction of this state level occupation growth rate measure.

Before continuing, it is important to discuss the level of occupation involved in this analysis. My measure of state-level occupation growth rates in this paper is at the most detailed level available in the Displaced Worker Survey, the Census occupation code. This classification, which comprises 324 Census occupation categories in the estimation sample, is more detailed than the Standard Occupation Classification (SOC) minor group (88 categories) or major group (10 categories). It is also much more detailed than the measure used in existing estimates of the occupation growth rate effects on displaced workers' outcomes (Carrington, 1993). I use these most detailed codes because it is not clear that occupations within a minor group would have the same growth rate. To see this, consider for example, Table 2.3, which lists examples of major, minor, broad and detailed SOC occupation categories, and Census occupation categories. Using one occupation growth rate for the minor group would assume that the growth rate of word processors and typists (43-9022) is the same as the growth rate of insurance claims and policy processing clerks (43-9031). My approach has the advantage of not imposing this assumption and allowing workers in different Census occupation categories to have different growth rates.

2.3.1 Construction of the State Occupation Growth Rate Measure

To proceed, I create an estimate of the occupation growth rate that does not use the OES as time series data. My decomposition is based on the fact that a given occupation's employment in a state is the sum of the occupation's employment in each of the state's industries.

More concretely, occupation o's employment in state s at year t, $E_{s,o,t}$, will be the sum of state employment in each industry j in that year, $E_{s,j,t}$, times the fraction of industry employment in that state and year that belongs to that occupation, $\alpha_{s,o,j,t}$.

$$E_{s,o,t} = \sum_j \alpha_{s,o,j,t} E_{s,j,t} \tag{2.1}$$

Because we are interested in growth rates, we can describe the change in occupational employment in state s and year t as

$$\Delta E_{s,o,t} = \sum_j \alpha_{s,o,j,t} E_{s,j,t} - \sum_j \alpha_{s,o,j,t-1} E_{s,j,t-1} \tag{2.2}$$

Unfortunately, equation 2.2 suffers from the limitations inherent using OES data for time series analysis, as both $\alpha_{s,o,j,t}$ and $\alpha_{s,o,j,t-1}$ come from adjacent years of the OES. For the reasons discussed earlier, this implies that the same employment data is used to determine these two estimates, and these estimates are not independent. However, assuming $\alpha_{s,o,j,t} = \alpha_{s,o,j,t-1} \forall t$, i.e. the share of occupation o in industry j in state s does not change over time, avoids this issue. This would be true if the production function of various goods and services and the costs of various types of labor are not changing over the sample period. Furthermore, this statement has empirical support – the correlation between national estimates of $\alpha_{o,j,2002}$ and $\alpha_{o,j,2013}$, the first and last year, is 0.9 in my sample. I use a fixed weight from the beginning of the sample to measure α, which I will refer to as $\alpha_{s,o,j,beginning}$.[8]

[8]The first year in which the North American Industry Classification (NAICS) is used in the OES data is 2002. I use a mean of 2002 and 2003 for the weight, although results are quite similar using only 2002, or some mean of years from the middle of the sample, say 2006-2008.

Then,

$$\widehat{\Delta E_{s,o,t}} = \sum_j (E_{s,j,t} - E_{s,j,t-1})\alpha_{s,o,j,beginning} \tag{2.3}$$

However, I'm interested in a growth rate, as opposed to a pure change in employment. A traditional growth rate measure would use adjacent years of occupation data, treating them as independent. However, recall that the adjacent years of occupation data are not actually independent. To avoid this problem, I use a fixed employment level at the beginning of the data period as the denominator for occupation o's growth rate in state s.

$$\frac{\widehat{\Delta E_{s,o,t}}}{E_{s,o,beginning}} = \frac{1}{E_{s,o,beginning}} \sum_j (E_{s,j,t} - E_{s,j,t-1})\alpha_{s,o,j,beginning} \tag{2.4}$$

$$= \sum_j \frac{E_{s,j,beginning}}{E_{s,o,beginning}} \frac{E_{s,j,t} - E_{s,j,t-1}}{E_{s,j,beginning}} \alpha_{s,o,j,beginning} \tag{2.5}$$

The state-level estimates are a function of two potentially noisy measures. It is possible to create variants of this occupation growth rate measure to decrease noise associated with certain state-level estimates. There are two reasons why state level estimates may be substantially noisier than national level estimates: noise in the industry growth rate and noise in the occupation-industry composition.

The 4 digit NAICS industry growth rate at the state level is fairly noisy. Over 14% of the state-industry-year cells have zero employees, and around 20% have fewer than 100 workers. Because of this characteristic, the state-level industry growth rate is highly variable for small industries and small states. Additionally, the displaced workers in my DWS sample may be directly affected by firms closing in their industries, heading to an endogeneity concern. To deal with these problems, researchers including

Autor and Duggan (2003a) have used national-level industry changes in employment, excluding the focal state's industry employment, which can be denoted by $E_{-s,j,t}$. This method, in the spirit of Bartik (1991), has two major advantages: first, it is not reliant on a single state's noisy industry employment, and second, it decreases the chance of a mechanical correlation between the displaced worker's job loss and the relevant employment conditions.

State-level occupation-industry composition suffers from a more significant limitation. Namely, the data only exists starting in 2012, and has been published as "research estimates." This designation implies a higher variability due to smaller samples. Additionally, these estimates are limited to state-occupation-industry cells with sufficient employment to disclose an estimate. As fewer estimates are withheld as employment numbers are aggregated to the national level, national estimates are available for far more occupation-industry cells and for every year in the sample.

Motivated by these concerns, my preferred estimate of the occupation growth rate uses national estimates of both the industry growth rate and occupation by industry composition:

$$\pi_{s,o,t} = \frac{\widehat{\Delta E_{s,o,t}}}{E_{s,o,beginning}} = \sum_j \frac{E_{s,j,beginning}}{E_{s,o,beginning}} \alpha_{o,j,beginning} \frac{E_{-s,j,t} - E_{-s,j,t-1}}{E_{-s,j,beginning}} \tag{2.6}$$

For clarity, the three components of the measure can be labeled as follows:

$$\pi_{s,o,t} = \frac{\widehat{\Delta E_{s,o,t}}}{E_{s,o,beginning}} = \sum_j \underbrace{\gamma_{s,o,j,beginning}}_{\substack{\text{State-specific}\\ \text{weight}}} \underbrace{\alpha_{o,j,beginning}}_{\substack{\text{Fraction}\\ \text{of occ } o\\ \text{in ind } j}} \underbrace{\frac{E_{-s,j,t} - E_{-s,j,t-1}}{E_{-s,j,beginning}}}_{\substack{\text{Growth rate of ind } j\\ \text{nationally}}}$$

I will use this measure, $\pi_{s,o,t}$, the predicted state-level occupation growth rate, condensed to "occupation growth rate" as my main regressor of interest for the remainder of the paper.

Figure 2.1(a) plots the distribution of occupation growth rates amongst the displaced workers in the sample. The mean worker-weighted occupation growth rate is -0.008 and the standard deviation is .04. Because this figure is less informative because of a small number of large (in absolute value) occupation growth rates at the tail, Figure 2.1(b) plots the distribution excluding the occupation growth rates above the 99th percentile and below the 1st percentile. The figures also show that the distribution is left-skewed.

For comparison, Figure 2.1(c) and (d) plot the distribution of industry growth rates, constructed analogously and discussed in greater detail below in Section 2.3.3. The mean industry growth rate is -0.01, and the standard deviation is 0.05 (a little larger than the standard deviation of the occupation growth rate at 0.05).

2.3.2 Estimation of the Impact of Occupation Growth Rates

I estimate the impact of the occupation growth rate on a displaced worker's labor market outcomes as follows:

$$Y_{i,s,o,t} = \beta\pi_{s,o,t} + \delta X_{i,s,o,t} + \lambda_s + \lambda_t + \varepsilon_{i,s,o,t} \tag{2.7}$$

where $\pi_{s,o,t}$ is the occupation growth rate, defined above. Each displaced worker is assigned an occupation growth rate based on their state of residence, occupation at displacement, and year of displacement. $X_{i,s,o,t}$ is a vector of individual characteristics including sex, race, education, years since displacement, indicators for different age categories, and a quadratic of tenure at the pre-displacement job. λ_s and λ_t are state of residence and year of displacement fixed effects. The primary outcomes of interest, $Y_{i,s,o,t}$, are the worker's re-employment status after displacement, occupation change, log duration of joblessness, and the change in log earnings. The regressions are weighted by the Displaced Worker Supplement Weights,[9] and standard errors are clustered at the state level. This regression specification compares two observationally identical displaced workers who have been displaced in the same state and same year from occupations growing at different rates.

The identifying assumption in equation (2.7) is that unobservable characteristics of displaced workers are uncorrelated with their occupation growth rate, conditional on observable individual characteristics, state, and year of displacement. This specification directly addresses the challenge of state workforce agencies, who are interested in targeting services to workers and need to decide between workers displaced in a state at similar times.

A potential disadvantage of the specification in equation (2.7) is that workers who select into different occupations may have different unobservable characteristics that affect labor market outcomes, which might be correlated with the occupation growth rate. This might be true, for example, if the most able workers recognize their occupation is shrinking, or vulnerable to shrinking, and change into more stable occupations. To allay concerns about differences in unobservable characteristics across displaced workers in different occupations, I also present estimates that add SOC minor group (3 digit) occupation fixed effects. This specification is as follows:

$$Y_{i,s,o,t} = \beta \pi_{s,o,t} + \delta X_{i,s,o,t} + \lambda_s + \lambda_t + \lambda_o + \varepsilon_{i,s,o,t} \tag{2.8}$$

These fixed effects control for the situation in which certain occupation categories have longer durations of joblessness or lower post-displacement earnings, independent of the occupation growth rate. In this specification, the variation is coming from differences

[9] Results are similar without weights.

within occupations, controlling for state and year fixed effects. The identifying assumption is that unobservable characteristics of the worker that affect their durations of joblessness and earnings losses are uncorrelated with their occupation growth rate, conditional on observable individual characteristics, pre-displacement occupation, state, and year of displacement. Of course, while alleviating concerns about bias, equation (2.8) relies on considerably less identifying variation, so it has a cost in terms of statistical power.

As the focus of this paper is displaced workers, I will discuss the magnitude of the effects for a one percentage point *decrease* in the occupation growth rate.

2.3.3 Comparison with Industry Growth Rate

Previous literature, including Carrington (1993), Kandilov (2010), and Crinò (2010), has found a significant effect of pre-displacement industry decline on displaced workers' labor market outcomes. However, there are few estimates of the relative impact of occupation growth compared to industry growth in the displaced workers' literature. Additionally, more state workforce agencies use historical data on changes in industry employment (59%) compared to historical data on changes in occupation employment (25%) in their prediction models (Dickinson et al., 1997).

To compare the impact of industry growth versus occupation growth on displaced workers' labor market outcomes, I run the following two regressions:

$$Y_{i,s,o,t} = \gamma \pi_{s,j,t} + \delta X_{i,s,o,t} + \lambda_s + \lambda_t + \varepsilon_{i,s,o,t} \tag{2.9}$$

$$Y_{i,s,o,t} = \gamma \pi_{s,j,t} + \beta \pi_{s,o,t} + \delta X_{i,s,o,t} + \lambda_s + \lambda_t + \varepsilon_{i,s,o,t} \tag{2.10}$$

where the first equation replaces the occupation growth rate with the state-level industry growth rate. The industry growth rate is analogously predicted from national industry growth.[10] This measure is constructed using the same approach as the occupation growth rate and therefore has the same advantages: it is not reliant on a single state's noisy industry employment, and it removes any chance of a mechanical correlation between the displaced worker's job loss and the relevant employment conditions.

Equation 2.10 adds the occupation growth rate back in. In this equation, the coefficient on occupation growth rate will be the impact of occupation growth holding industry growth constant. Similarly, the coefficient on industry growth rate will be the impact of industry growth holding occupation growth constant.

The labor market outcomes discussed in this context are the log duration of joblessness and change in log earnings. Industry growth is at the three digit NAICS level.

2.4 Results

2.4.1 Variation in the Occupation Growth Rate

Recent research on Bartik instruments by Goldsmith-Pinkham et al. (2018) finds that a number of empirical applications rely heavily on a few industries for identifying variation. In my dataset, this is not the case. The variation in the occupation growth rate in my data is calculated from 291 NAICS 4 digit industries. Figure 2.2 displays a histogram of the number of industries used to derive each occupation growth rate. The figure highlights a key descriptive statistic of the paper: most displaced workers' occupations exist in a wide number of industries. The mean (median) worker in the sample has an occupation growth rate that is a function of 125 (124) industries. In the extreme, if each occupation is represented in one industry, the occupation growth rate is exactly equal to the industry growth rate. The figure provides one piece of evidence that

[10] More formally, $\pi_{s,j,t} = \frac{E_{-s,j,t} - E_{-s,j,t-1}}{E_{-s,j,beginning}}$

suggests the occupation and industry growth rate may differ in meaningful ways.

The occupation growth rate also varies substantially over this time period (2001 - 2013). Figure 2.3 compares the minimum and maximum occupation growth rates for state-occupation combinations represented in my displaced workers' sample (i.e. the within-state and occupation across time variation I exploit). The mean difference between the minimum and maximum growth rate is 0.10 and the standard deviation is 0.07.

A second potential problem is that these industries may have characteristics that are correlated with observables, which may suggest potential unobserved confounders. In my context, these are not concerns for the following reasons: First, the fact that different occupations are concentrated in different industries and to different extents is a characteristic of the labor market and an important empirical fact. Second, if a few industries in an occupation are driving occupation growth, occupation growth and industry growth would be highly correlated. The resulting coefficients and standard errors from the regression model would take into account the correlation between the growth rates through the covariance terms in the standard errors. This empirical finding would have an implication for the relative importance of occupation and industry specific human capital – namely, that because of the structure of the labor market, it is difficult to separately identify the effects of occupation and industry specific human capital, and the distinction between the two may be unnecessary.

Because my context is not an instrumental variables context, I cannot measure the sensitivity-to-misspecification elasticity as recommended in the Goldsmith-Pinkham et al. (2018). Finally, in the Robustness section, I show that the estimates are similar when excluding one industry at a time.

2.4.2 The Effect of the Occupation Growth Rate

Table 2.4 shows the effect of the pre-displacement occupation growth rate on the probability of working for pay after displacement, controlling for elapsed time between displacement and the survey date. As the DWS only asks calendar year of displacement, this is only a rough control for elapsed time.[11] Working for pay is assumed for workers currently employed, and asked of individuals who are both unemployed and not in the labor force. Approximately 71 percent of the sample had worked for pay by the time they were surveyed. In Column (1), the specification with state and year of displacement fixed effects, the occupation growth rate does not have a statistically significant, or economically significant, relationship with working for pay after displacement. The biggest determinant of working since displacement is the time elapsed since displacement – workers who were displaced three (two) years ago are approximately 27 (9) percentage points more likely to have worked for pay after displacement, respectively, compared to workers displaced one year ago. The other coefficients in this regression follow expected patterns – older workers are less likely to work after displacement, more educated workers are more likely to work after displacement. In Column (2), the specification adding minor group occupation fixed effects, the occupation growth rate continues to have an insignificant relationship with working for pay after displacement.

The next outcome is log duration of joblessness. Duration of joblessness is defined as the number of weeks that went by between displacement and when the respondent started working again. This is self-reported by all displaced workers who have worked for pay at some time since displacement.[12] For other workers, those who have not worked for pay since displacement, the DWS unfortunately does not ask duration of joblessness.

[11] Workers are asked which calendar year they were displaced in January. In all regressions, I include indicators for two calendar years ago and three calendar years ago, with the omitted category being one calendar year ago.

[12] It is topcoded at 100 weeks, although this affects a small fraction (2%) of the sample.

Because the DWS only asks year of displacement, any statements about jobless durations of workers who are not re-employed are highly imprecise.Thus I omit these workers from my main estimates, working with the sample of self-reported completed durations only.[13] Approximately 70 percent of workers have worked for pay after displacement, and as discussed earlier, ever working for pay after displacement is largely a function of time elapsed since displacement. In the Robustness section, I report the results from censored duration regressions that include non-re-employed workers under various assumptions for calculating their incomplete durations.

Table 2.5 Column (1) shows that a one percentage point decrease in the growth rate of a worker's occupation in the state and year of displacement is associated with a 4.5 percent increase in the duration of joblessness conditional on having been re-employed after displacement. The estimate is similar with minor group occupation fixed effects in Column (2) – a one percentage point decrease is associated with a 3.9 percent increase in the duration of joblessness. This translates into a one standard deviation decrease (approximately four percentage points) is associated with a 16.1 percent increase in the duration of joblessness conditional on having been re-employed after displacement.

Previous literature by Poletaev and Robinson (2008) and Kambourov and Manovskii (2009) has focused extensively on the correlation between occupation change and displaced workers' earnings and employment outcomes. But under what conditions do displaced workers change occupations? Table 2.6 analyzes the effect of the occupation growth rate on the probability of an occupation change for workers who are currently employed. The majority of workers in the sample (64 percent) change occupations after displacement. Linear probability models with occupation change as the dependent variable are displayed in Column (1), and show that a one percentage point decrease in the occupation growth

[13]As in most censored regression contexts, I expect the exclusion of these incomplete durations (which will be longer, on average) to attenuate my estimates of occupation growth rates on duration of joblessness. Indeed, this is what I find.

rate is associated with a 0.95 percentage point increase in the probability of an occupation change. Column (2) adds minor group occupation fixed effects, which decrease the magnitude of the point estimate but the new estimate is not statistically different. It appears that workers change occupations because their occupations are shrinking – a one standard deviation lower occupation growth rate is associated with a 3.7 to 4.1 percentage point (5.7-6.4 percent) increase in the probability of changing occupations. This is a large effect of temporary conditions in a worker's pre-displacement occupation.

Table 2.7 looks at the change in log earnings between pre- and post-displacement jobs, conditional on re-employment. Earnings changes are related to the worker's occupation growth rate: a one percentage point decrease in the occupation growth rate is associated with a 1.5 percent decrease in post-displacement earnings. This effect is similar when adding minor goup occupation fixed effects – a one percentage point decrease in the occupation growth rate is associated with a 2.2 percent decrease in post-displacement earnings. The standard deviation of the occupation growth rate amongst this sample is 0.042, suggesting that a worker who is displaced in conditions one standard deviation below the mean suffers, all else equal, a 9.2 percent larger earnings loss than a worker displaced in conditions at the mean.

2.4.3 Comparison with Industry Growth

With a clear understanding of the negative impact of the occupation growth rate on displaced workers' labor market outcomes, I now turn to understanding its role relative to the industry growth rate. Before presenting regression results on these relationships, I begin by showing descriptive evidence. In Figure 2.4, the occupation and industry growth rate are split into five categories: shrinking substantially, shrinking, neutral, growing slightly, and growing substantially. The mean and 95% confidence interval is displayed for workers in each of these categories. In Figure 2.4(a), the range spanned by the means and confidence intervals of the occupation growth rate is larger than the range spanned by the means and confidence intervals of the industry growth rate. A similar story appears in Figure 2.4(b), the equivalent figure for change in log earnings. Workers displaced when their occupation is shrinking substantially have larger earnings losses than when their occupation is growing substantially ($p = 0.0757$). This is not true of workers displaced when their industry is shrinking substantially compared to workers displaced when their industry is growing substantially ($p = 0.3938$). However, these results can be driven only by differences amongst workers in these samples and/or state and year of displacement characteristics. Table 2.8 more formally tests the relative effects of occupation and industry growth rates.

Table 2.8 Panel A compares the impact of occupation versus industry growth on the duration of joblessness. Column (1) shows that a one percentage point decrease in the occupation growth rate is associated with a 4.5 percent longer duration of joblessness. Column (2) shows that a decrease in the industry growth rate has a smaller effect on duration of joblessness, but still lengthens it – a one percentage point decrease in the industry growth rate is associated with a 2.1 percent increase in the duration of joblessness. These coefficients, in Columns (1) and (2), are also statistically different. Column (3) includes both the occupation and industry growth rates in the same regression. While the magnitude of the occupation growth rate shrinks slightly, it is still statistically and economically significant. The industry growth rate coefficient is also smaller, and now not statistically significant. The relative magnitudes here are important – the point estimate on the occupation growth rate is ten times the size of the point estimate on the industry growth rate. The test of equality between the two coefficients in the regression shows that we can reject the null hypothesis that these growth rates are the same at the 1% level.

Columns (4) - (6) add minor group occupation fixed effects to the specifications in Columns (1) - (3). These fixed effects do not significantly change the magnitude of the estimates. A one percentage point decrease in the occupation growth rate is associated with a 3.9 percent longer duration of joblessness. On the other hand, the effect of a one percentage point decrease in the industry growth rate is a statistically insignificant 1.2 percent. Column (6) shows the "horse race" regression specified in Equation 2.8. Again, the effect of the occupation growth rate is much larger than the effect of industry growth rate. As in Column (3), the industry growth rate has a quite small impact on duration of joblessness. It is valuable to remember that the occupation growth rate is constructed using industry growth rates. As such, it should not be surprising that the correlation between the occupation growth rate and the industry growth rate is 0.70 in this sample. Despite this fact, the estimated occupation growth rate effect is statistically different in both comparisons in Table 2.8.

Table 2.8 Panel B compares the impact of industry and occupation growth on the displaced workers' change in log earnings. A one percentage point decrease in the occupation growth rate is associated with a 1.5 percent decrease in earnings. A one percentage point decrease in the industry growth rate is associated with a 0.9 percent decrease in weekly earnings. The coefficient on the occupation growth rate is larger, though not significantly larger, than the coefficient on industry growth rate. When the occupation growth rate and industry growth rate are in the same regression, as in column (3), neither effect is statistically significant although we can reject the null hypothesis that they are jointly equal to zero ($p = 0.0037$). A similar story can be told with occupation fixed effects in columns (4)-(6).

2.5 Robustness

This section assesses the robustness of these results to a variety of potential concerns.

The first concern is whether the occupation growth rate used is truly a measure of temporary conditions at the time of displacement, or if it reflects something systematic about the occupational labor market. If the effect is truly the effect of displacement during poor labor market conditions, then it should be largest for the contemporaneous occupation growth rate, and not the occupation growth rate in years prior to displacement. Table 2.9 compares the specification based on the contemporaneous occupation growth rate with the occupation growth rate last year, the occupation growth rate two years ago, and the mean occupation growth rate in the three years leading up to the displacement (the contemporaneous year, the year prior and two years prior). To make comparisons across these four specifications, the sample is limited to workers who have all four measures, decreasing the sample size by excluding workers displaced in 2001 and 2002. The contemporaneous growth rate, in Table 2.9 Panel A Columns (1) and (5), has the largest effect on the worker's duration of joblessness, followed by the mean growth rate. Panel B, which changes the focus to change in log earnings, provides more support for the contemporaneous growth rate. Here, the contemporaneous growth rate is the only statistically significant estimate. The mean growth rate, in this case, even has the 'wrong' sign (Column 8).

To ensure that this effect is truly the effect of temporary labor market conditions at displacement, I run a placebo test, comparing the effect of the contemporaneous occupation growth rate with the effect of the occupation growth rate four years after displacement on the worker's duration of joblessness. The sample is limited to workers for whom both contemporaneous and four year later occupation growth rates are available, and therefore, workers displaced after 2010 are excluded. By four years after the reported calendar year of displacement, the vast majority of displaced workers have been re-employed, and therefore the occupation growth rate should have little effect on the worker's duration of joblessness. In Table 2.11, the occupation growth rate four years after displacement has a significant effect on duration of joblessness in the absence of controls for occupation. This is surprising as the occupation growth rate four years later is negatively correlated with the contemporaneous occupation growth rate ($\rho = -0.2385$). In Column (4), which adds minor group occupation fixed effects, the estimate of the occupation growth rate four years later becomes much smaller and is statistically insignificant.[14]

As previously discussed, recent literature on shift-share approaches by Goldsmith-Pinkham et al. (2018) suggests that many empirical applications rely on a few industries for identifying variation. If one industry is driving the results, it also may be true that this industry's growth or decline is not exogenous to the worker and/or is correlated with the unobservable characteristics related to the worker's labor market conditions. To show that the occupation growth rate is not being driven by a singular industry's employment changes, I construct the occupation growth rate leaving out one industry at a time, then run the main regressions with these new "leave-one-out" growth rates. The result is 291 estimates of the occupation growth rate's effect on each outcome. Figure 2.5 displays histograms of the estimates for the four primary outcomes: log duration of joblessness

[14] While it seems plausible to expect no effects of the growth rate of a worker's pre-displacement occupation four years after displacement on the length of the worker's first post-displacement jobless spell (an outcome that is most likely determined by that time), a similar null effect does not seem likely on the worker's earnings at the DWS survey date, which can be up to three years after displacement. Employment growth (estimated by the growth of occupation o between $t+3$ and $t+4$) in the pre-displacement occupation should still be correlated with the worker's outside options, since he or she has skills related to that occupation, for reasons explored by Beaudry et al. (2012) and Tschopp (2017). For these reasons, Table 2.11 type regressions do not constitute a valid placebo test when earnings are the outcome of interest. Interestingly, while the standard errors are large, four-year-later occupation growth rates in those regressions have consistently positive coefficients, regardless of whether the worker has switched occupations. I interpret this as suggestive evidence of search and bargaining effects in local labor markets.

and change in log earnings, with and without occupation fixed effects. The original estimate of the occupation growth rate (with all industries) is denoted by a dashed line. The 95 percent confidence interval of "leave-one-out" estimates is denoted by solid lines. From these figures, it is clear that the occupation growth rate is insensitive to any single industry's employment.

The occupation growth rate, however, is constructed such that is mechanically correlated with the industry growth rate. While the above robustness check suggests that this mechanical correlation is unlikely to drive the results, it may impede the interpretation of the coefficients as representing independent factors. To address this concern, I develop an alternative occupation growth rate measure, defined as

$$\pi_{s,o,-j,t} = \frac{\widehat{\Delta E_{s,o,t}}}{E_{s,o,beginning}} = \sum_{j!=j'} \underbrace{\gamma_{s,o,j,beginning}}_{\substack{\text{State-specific}\\ \text{weight}}} \underbrace{\alpha_{o,j,beginning}}_{\substack{\text{Fraction}\\ \text{of occ } o\\ \text{in ind } j}} \underbrace{\frac{E_{-s,j,t} - E_{-s,j,t-1}}{E_{-s,j,beginning}}}_{\substack{\text{Growth rate of ind } j\\ \text{nationally}}} \tag{2.11}$$

where the weighted average intentionally excludes industry j, the industry that displaced worker in the sample was displaced from. To put this more concretely, the millwright who was displaced from a transportation equipment manufacturing plant has an occupation growth rate that is a function of all the industries in his state, excluding transportation equipment manufacturing. The estimation equation, therefore, is

$$Y_{i,s,o,t} = \gamma\pi_{s,j,t} + \beta\pi_{s,o,-j,t} + \delta X_{i,s,o,t} + \lambda_s + \lambda_t + \varepsilon_{i,s,o,t} \tag{2.12}$$

The coefficient on $\pi_{s,o,-j,t}$ is expected to be smaller in magnitude than the preferred specification, as this new measure excludes the growth of the industry that that displaced worker is most likely to find work in.

Table 2.10 suggests that the mechanical correlation was not driving the results.

The coefficients on the newly constructed occupation growth rate are similar to the prior specifications, e.g. one percentage point decrease in the occupation growth rate is associated with a 5.1 - 5.6 percent increase in duration of joblessness. The test for equality row suggests that despite this handicap on the occupation growth rate, it continues to outperform the industry growth rate in its relevance for duration of joblessness. The results for change in log earnings are similar, but without much precision. Unsurprisingly, the standard errors on the estimates are larger, and the new correlation between occupation and industry growth rates is smaller ($\rho = 0.515$).

Another concern may be the sensitivity to the way the occupation growth rate is specified. To address this concern, I vary the method by which I estimate the occupation growth rate. Table 2.12 shows the effect of the occupation growth rate measured in four different ways on log duration of joblessness and change in log earnings. As discussed in section 4.1, my preferred measure of the occupation growth rate uses Equation 2.6, displayed in Columns (1) and (2) of Table 2.12. The three components of this measure are the state-specific weight and national estimates of the industry growth rate and national estimates of occupation by industry composition. To show robustness to different measures, I replace the national occupation by industry composition term with a state-specific occupation by industry composition term. This comes from the OES research estimates of state-level occupation by industry employment, which started in 2012. In other words, I replace $\alpha_{o,j,beginning}$ with $\alpha_{s,o,j,2012}$ in Equation 2.6. This estimate is displayed in Column (3) and (4) in Table 2.12 with and without occupation fixed effects. The estimate is smaller than the corresponding estimates using the national occupation by industry composition term but still statistically significant.

The next measure of occupation growth rate returns to Equation 2.6 and replaces national industry growth with state s's industry growth. This estimate is displayed in Columns (5) and (6). The estimate is smaller than the estimates from Column

(1) and (2) but still economically meaningful (notably, bigger than the estimates of the industry growth rate from 2.8). Finally, in Columns (7) and (8), the occupation growth rate measure combines the two changes. This estimate is the smallest of the four, but still statistically significant. The pattern is similar when considering changes in weekly earnings, though the results become insignificant when using the own state industry growth rate. The weaker estimates when using state level industry growth may be evidence of greater measurement error in these values.

As the Displaced Workers Survey does not ask duration of joblessness for individuals who have not been re-employed by the CPS survey date, my main results on duration of joblessness did not include those who have not been re-employed. While Table 2.4 shows that the occupation growth rate does not have a significant effect on the probability of working for pay after displacement, a concern may be that my estimated effects of the occupation growth rate are affected by right-censoring of observed durations. This concern is addressed in Table 2.13, which demonstrates robustness of the log duration of joblessness result by including workers who have not been re-employed. The DWS asks respondents the calendar year they were displaced. Workers who are not re-employed at the time of the survey will have been jobless for some minimum amount of time. To incorporate this information, I first treat all workers with incomplete durations as being displaced in the middle of their displacement year. Then, workers who were displaced one, two and three years ago have minimum durations of 26, 78, and 130 weeks, respectively. I then include these minimum durations in a right-censored regression that includes both complete and incomplete spells. The results are reported in Table 2.13. In Column (1), a one percentage point decrease in the occupation growth rate is associated with a 3.9 percent increase in duration of joblessness, an effect that is similar to the estimated 4.5 percent in Table 2.5. This result is also quite robust to supposing that all workers were displaced in any other month of the year: the estimates range between -3.84 and

-4.20. In Column (2), the specification with occupation fixed effects, the estimate is however much smaller and statistically insignificant. Since these censored regressions must be estimated by maximum likelihood, the low power of this estimate may reflect an incidental parameters problem associated with the large number of occupation fixed effects.

Finally, I test the robustness of the results for duration of joblessness and earnings losses to different functional forms of the dependent variable. For duration of joblessness, the functional forms I consider are commonly used by various State Workforce Agencies in their profiling systems. Table 2.14 Panel A Columns (1) and (2) look at levels of duration of joblessness, instead of logs, and thus includes workers who have 0 weeks of joblessness. A one percentage point lower occupation growth rate is associated with 0.64 to 0.74 weeks longer duration of joblessness, similar to the effect in Table 2.5. The Displaced Workers Survey asks those who said they claimed unemployment benefits whether they exhausted their unemployment benefits. This outcome is displayed in Columns (3) and (4). The sample size is smaller for this group as workers may not have taken unemployment benefits for various reasons. Though only marginally significant in one specification, this outcome provides some suggestive evidence that the occupation growth rate is related to exhausting formal unemployment benefits. Column (5) and (6) attempt to proxy for a binary indicator of unemployment duration – the most common measure used by State Workforce Agencies. As the Displaced Workers Survey does not explicitly ask for the duration of unemployment, I proxy for duration of unemployment with duration of joblessness. I create a proxy for the fraction of benefits exhausted by dividing the duration of joblessness by 26, with a maximum value of one. Workers who were displaced two or three years ago and have not worked since displacement were also considered to be jobless for more than 26 weeks. While the maximum number of weeks for which unemployment benefits were provided changed during this time period, the

DWS does not provide detailed timing information, and so this is a rough proxy. A one percentage point decrease in the occupation growth rate is associated with a .008 to .012 percentage point (2.4 percent to 3.6 percent) increase in the probability of having 26 or more weeks without a job. Overall, this panel provides further evidence supporting the claim that the occupation growth rate is a predictor of the duration of joblessness and suggests that states looking to improve their targeting of displaced workers should use this information.

Table 2.14 Panel B tests the robustness of the earnings results to functional form, and including workers who have not been re-employed. Instead of using change in log weekly earnings, Panel B uses change in weekly earnings. The results using levels in Columns (1) show that a one percentage point decrease in the occupation growth rate is associated with a \$6.24 decrease in weekly earnings. Given the mean lost job earnings in this sample, this translates to a 0.8 percent decrease at the mean, a little smaller than the 1.5 percent change in Table 2.7. The effect is larger when including minor group occupation fixed effects in Column (2), a one percentage point decrease is associated with a \$11.53 decrease in weekly earnings, or a 1.5 percent decrease at the mean. This specification, using levels instead of logs, has the added advantage of including the entire lost job earnings of displaced workers who have not been re-employed. In Columns (3) and (4), I include these workers. Unsurprisingly, the mean earnings loss in this sample is much higher. The result without occupation fixed effects, in Column (3), is similar. However, in Column (4), the result is no longer significant because of the large standard errors. Further inspection of the data suggests that these large standard errors are driven by outliers – some workers who had extremely high lost job earnings prior to displacement have not been re-employed, while other workers experienced massive earnings gains after displacement. The change in log earnings outcome was not as sensitive to these outliers. Adding a restriction to exclude workers at the bottom 1st percentile or top 99th percentile of the change in weekly earnings distribution yields estimates similar to the estimate in Column (1). This assurance that the results are not incredibly sensitive to functional form, and hold when including most workers who have not been re-employed, supports the claim that the occupation growth rate affects displaced workers' earnings losses.

2.6 Conclusion

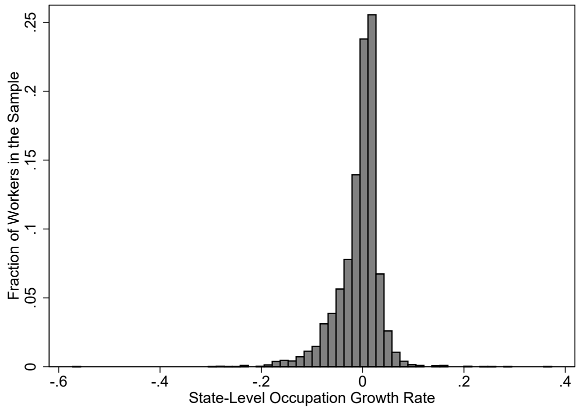
Policymakers and researchers have contemplated causes of displaced workers' earnings losses and attempted to identify vulnerable displaced workers. While it is well established that older and high tenure workers lose more, much less is known about other determinants of displaced workers' earnings losses. This is additionally problematic as recent literature has focused on ex post determinants of earnings losses, such as industry and occupation change, which do not have a clear causal interpretation and are less helpful to states' worker-targeting decisions. While the role of occupation growth is quite intuitive, data limitations have hindered this analysis.

This paper shows that the growth rate of a displaced worker's pre-displacement occupation significantly impacts that worker's duration of joblessness and earnings losses. A one standard deviation decrease in the worker's occupation growth rate (which is approximately four percentage points) is associated with a 16.1 percent increase in the duration of joblessness and a 9.2 percent decrease in weekly earnings. These are large effects associated with temporary conditions in the worker's pre-displacement occupation. Importantly, the effect of industry growth holding occupation growth constant is quite small in comparison. This implies that workers' difficulties are less related to the goods and services they were producing, and more related to the activities they were performing at work.

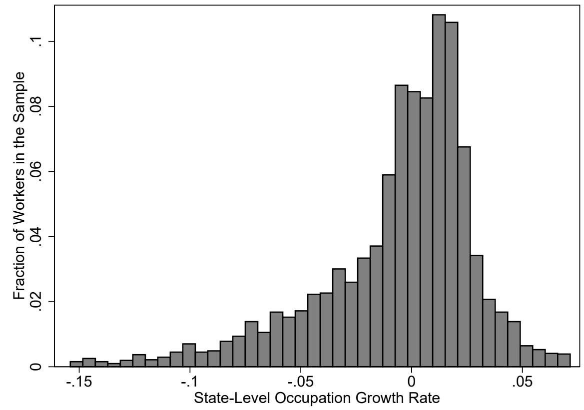
This result suggests a significant role of occupation specific human capital in determining displaced workers' earnings. Notably, it is similar to the short-run effect of graduating during a typical recession. It is larger than the effect found in recent work by Lachowska et al. (2018) of losses attributable to foregone employer fixed effects amongst workers displaced from employers paying top-quintile earnings premiums. This suggests a greater role of occupation specific human capital compared to firm-level rents in the debate on theories of displaced workers' earnings losses. Finally, the large effect of a temporary employment decline in one's own occupation suggests that workers are not well-adapted to doing different work. Notably, the result in this paper applies to a more representative sample of occupations and a broad measure of employment conditions, in contrast to previous work that is highly focused on the manufacturing sector or on labor market shocks related to trade.

Our current social insurance system is not targeting assistance based on occupation. In fact, not all states even collect the occupation of unemployment insurance claimants. As new technology has the potential to fundamentally affect the labor market and it appears that workers of different occupations will be affected differently (Brynjolfsson et al., 2018), this information may be increasingly useful in improving the provision of scarce resources for re-employment assistance, based on information available to the states at the time of displacement.

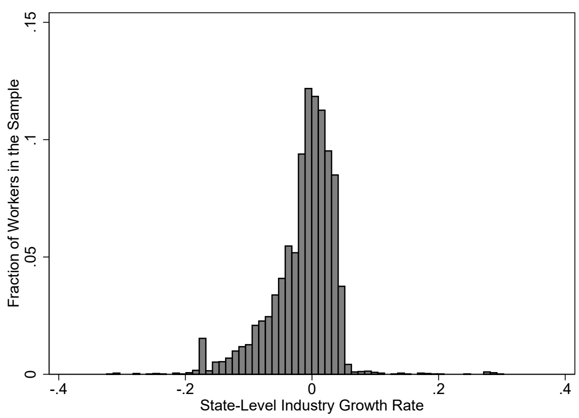
Figure 2.1: Distribution of Pre-Displacement Occupation Growth Rates for Workers Displaced From Full-Time Jobs



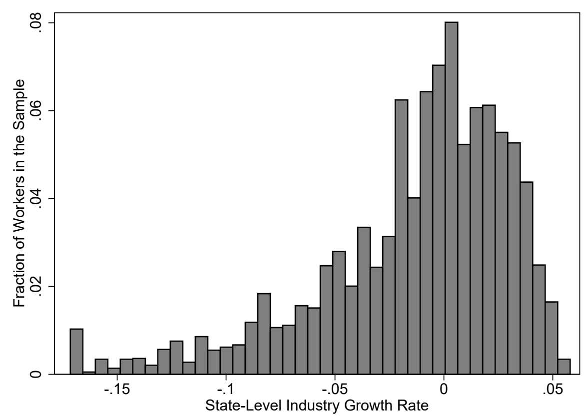
(a) Distribution of Occupation Growth Rates



(b) Distribution of Occupation Growth Rates - Excluding growth rates below the 1st percentile and above the 99th percentile



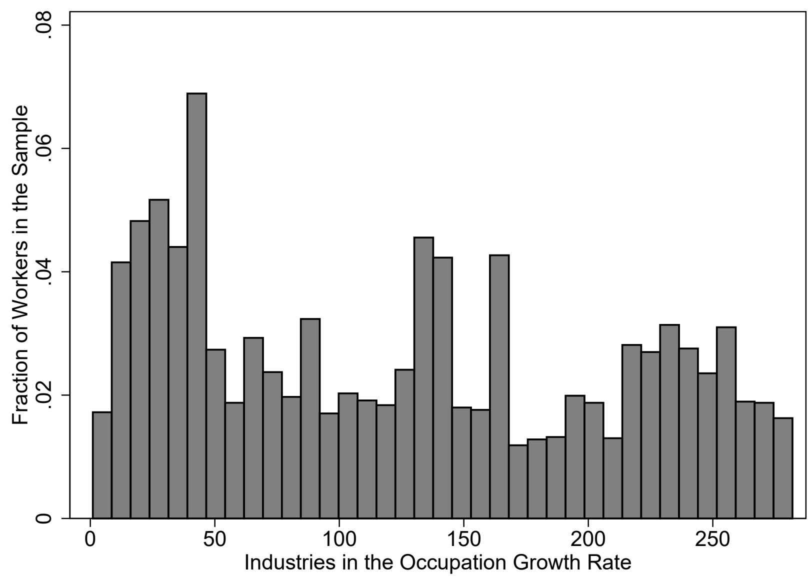
(c) Distribution of Industry Growth Rates



(d) Distribution of Industry Growth Rates - Excluding growth rates below the 1st percentile and above the 99th percentile

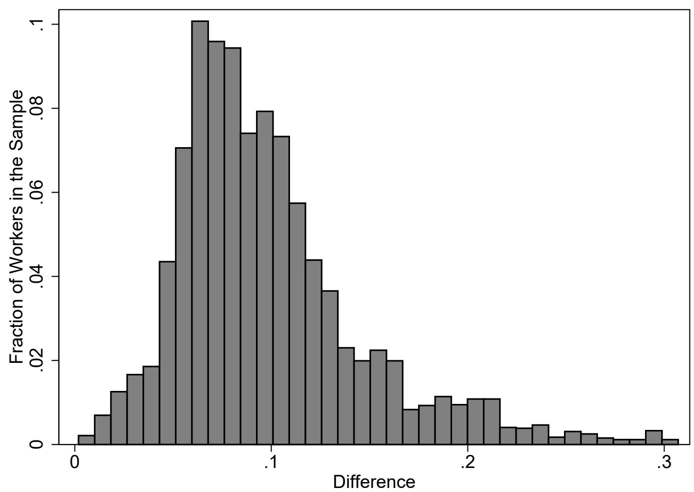
Notes: The sample is limited to workers responding to the Current Population Survey Displaced Workers Survey Supplement between 2004 and 2014. Workers have been displaced because their plant or firm closed or moved, and be displaced from a full-time occupation. Workers in this sample must report pre-displacement occupation or industry and have a pre-displacement occupation or industry growth rate.

Figure 2.2: Distribution of the Number of Industries Used to Estimate Each Worker's Occupation Growth Rate



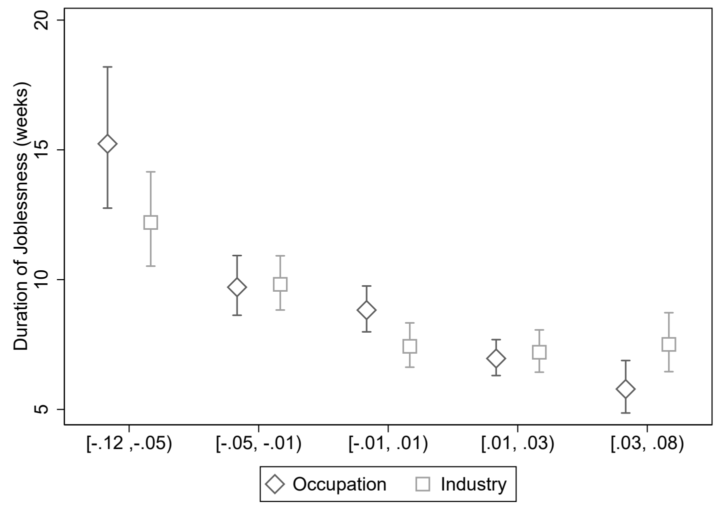
Notes: This figure displays the number of 4-digit NAICS industries affecting each worker's occupation's employment. The observation is at the worker level, limited to workers responding to the Current Population Survey Displaced Workers Survey Supplement between 2004 and 2014. Workers have been displaced because their plant or firm closed or moved, and be displaced from a full-time occupation. Workers in this sample must report pre-displacement occupation and have a pre-displacement occupation growth rate.

Figure 2.3: Distribution of Differences between Highest and Lowest Growth Rate within Occupation-State Cells

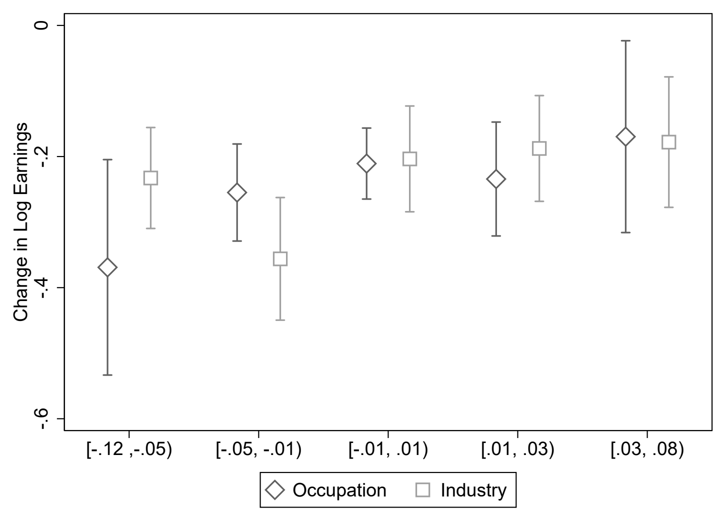


Notes: The sample is limited to occupation-state combinations that correspond to workers responding to the Current Population Survey Displaced Workers Survey Supplement between 2004 and 2014. Workers have been displaced because their plant or firm closed or moved, and be displaced from a full-time occupation and report their occupation.

Figure 2.4: Estimates by Bins of Occupation and Industry Growth Rates



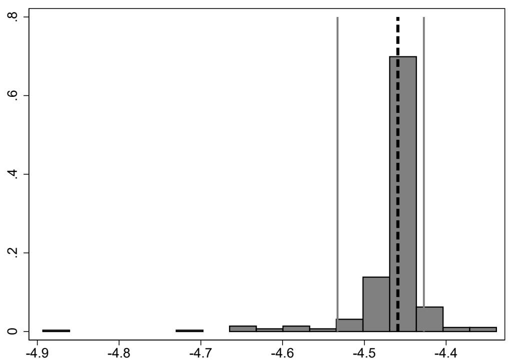
(a) Duration by Bins of Occupation and Industry Growth Rates



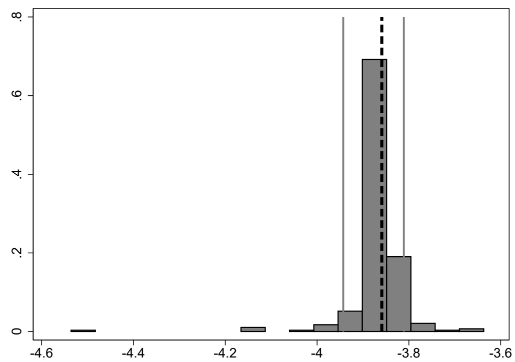
(b) Earnings Changes by Bins of Occupation and Industry Growth Rates

Notes: This figure displays coefficients and 95 percent confidence intervals by bin of the occupation growth rate and industry growth rate from the regressions on log duration of joblessness (on the top) and change in log earnings (on the bottom). Workers outside of this range of industry or occupation growth rates are excluded. Workers have been displaced because their plant or firm closed or moved, and be displaced from a full-time occupation and report their occupation and industry and have a corresponding occupation and industry growth rate.

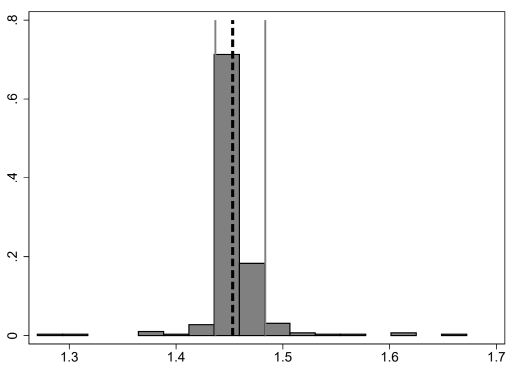
Figure 2.5: Leave One Out Estimates of the Effect of Occupation Growth Rate



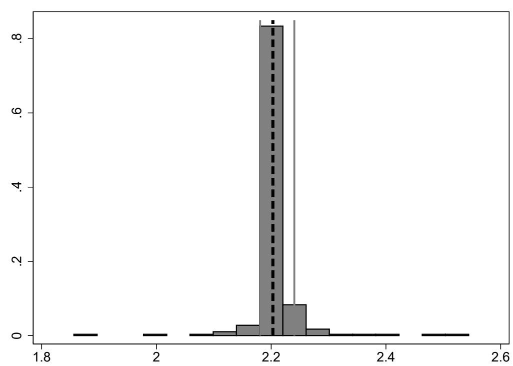
(a) Duration with State and Year FE



(b) Duration with State, Year, and Occupation FE



(c) Δ Earnings with State and Year FE



(d) Δ Earnings with State, Year, and Occupation FE

Notes: This figure displays regression estimates based on an occupation growth rate leaving one industry out from the construction of the occupation growth rate. Sample restrictions are the same as Tables 2.5 and 2.7. Subfigures (a) and (b) plot the effect of these “leave-one-out” occupation growth rates on log duration of joblessness, and subfigures (c) and (d) plot the effect of these “leave-one-out” occupation growth rates on change in log earnings. Subfigures (a) and (c) include state and year fixed effects, and (b) and (d) include state, year, and occupation fixed effects. The solid lines correspond to the 95 percent confidence interval of these estimates. The dashed line corresponds to the estimate including all industries, displayed in Tables 2.5 and 2.7.

Table 2.1: Summary Statistics

Less Than HS	0.128
	(0.335)
HS Diploma	0.357
	(0.479)
Some College	0.292
	(0.455)
BA/BS	0.167
	(0.373)
Graduate Degree	0.0552
	(0.228)
White	0.794
	(0.405)
Black	0.136
	(0.342)
Asian	0.00952
	(0.0971)
Other Race	0.0612
	(0.240)
Age	42.82
	(12.18)
Tenure	6.750
	(7.758)
Female	0.419
	(0.493)
Displaced 3 Years Ago	0.325
	(0.468)
Displaced 2 Years Ago	0.305
	(0.460)
Displaced Last Year	0.371
	(0.483)
Change in Weekly Earnings	-108.1
	(465.7)
Change in Log Earnings	-0.253
	(0.909)
Worked for Pay Since Displacement	0.704
	(0.457)
Duration of Joblessness in Weeks	15.73
	(21.75)
Year Displaced	2006.6
	(3.508)
Occupation Growth Rate	-0.00792
	(0.0430)
Observations	5975

Notes: The sample is limited to workers responding to the Current Population Survey Displaced Workers Survey Supplement between 2004 and 2014. Workers have been displaced from a full-time job because their plant or firm closed or moved. Workers in this sample must report pre-displacement occupation, full-time status before displacement, and whether they moved after displacement. Summary statistics are weighted using Displaced Workers Survey Supplement weights. Means are reported, and standard deviations are in parentheses.

Table 2.2: Differences between workers reporting and not reporting occupation

Sample	Analysis Data (1)	Missing (2)	Difference (3)
Less Than HS	0.128	0.166	-0.038
	(0.335)	(0.372)	(0.013)
HS Diploma	0.357	0.358	-0.000
	(0.479)	(0.480)	(0.016)
Some College	0.292	0.279	0.013
	(0.455)	(0.448)	(0.015)
BA/BS	0.167	0.157	0.011
	(0.373)	(0.364)	(0.012)
Graduate Degree	0.055	0.040	0.015
	(0.228)	(0.197)	(0.007)
White	0.794	0.759	0.035
	(0.405)	(0.428)	(0.015)
Black	0.136	0.175	-0.040
	(0.342)	(0.381)	(0.014)
Asian	0.010	0.012	-0.002
	(0.097)	(0.108)	(0.004)
Other Race	0.061	0.054	0.007
	(0.240)	(0.226)	(0.008)
Age	42.817	42.783	0.034
	(12.179)	(13.516)	(0.451)
Tenure	6.750	4.604	2.146
	(7.758)	(5.590)	(0.305)
Female	0.419	0.414	0.005
	(0.493)	(0.493)	(0.017)
Displaced 3 Years Ago	0.325	0.253	0.072
	(0.468)	(0.435)	(0.018)
Displaced 2 Years Ago	0.305	0.305	0.000
	(0.460)	(0.460)	(0.019)
Displaced Last Year	0.371	0.443	-0.072
	(0.483)	(0.497)	(0.021)
Change in Weekly Earnings	-108.134	80.517	-188.651
	(465.743)	(403.863)	(40.803)
Change in Log Earnings	-0.253	0.067	-0.320
	(0.909)	(1.007)	(0.098)
Worked for Pay Since Displacement	0.704	0.804	-0.100
	(0.457)	(0.397)	(0.015)
Duration of Joblessness in Weeks	15.733	10.213	5.520
	(21.751)	(14.420)	(1.083)
Year Displaced	2006.622	2007.471	-0.849
	(3.508)	(3.454)	(0.142)
Observations	5975	1128	7103

Notes: This table compares workers who have been displaced from a firm or plant closing who do report (Column 1) and do not report (Column 2) key variables – pre-displacement occupation, year of displacement, full-time status at pre-displacement occupation, and whether the worker moved after displacement. The standard deviations are reported in parentheses. Column (3) is the difference between the two columns, with the standard error of the difference in parentheses.

Table 2.3: Examples of Occupation Categorization

SOC Major	SOC Minor	SOC Broad	SOC Detailed	Census Code	
43-0000					Office and Administrative Support Occupations
	43-6000				Secretaries and Administrative Assistants
	43-9000				Other Office and Administrative Support Workers
		43-9010			Computer Operators
			43-9011	5800	Computer Operators
		43-9020			Data Entry and Information Processing Workers
			43-9021	5810	Data Entry Keyers
			43-9022	5820	Word Processors and Typists
		43-9030			Desktop Publishers
			43-9031	5830	Desktop Publishers
		43-9040			Insurance Claims and Policy Processing Clerks
			43-9041	5840	Insurance Claims and Policy Processing Clerks
		43-9050			Mail Clerks and Mail Machine Operators, Except Postal Service
			43-9051	5850	Mail Clerks and Mail Machine Operators, Except Postal Service
		43-9060			Office Clerks, General
			43-9061	5860	Office Clerks, General
		43-9070			Office Machine Operators, Except Computer
			43-9071	5900	Office Machine Operators, Except Computer
		43-9080			Proofreaders and Copy Markers
			43-9081	5910	Proofreaders and Copy Markers
		43-9110			Statistical Assistants
			43-9111	5920	Statistical Assistants
		43-9190			Miscellaneous Office and Administrative Support Workers
			43-9199	5930	Office and Administrative Support Workers, All Other

Notes: This table is an example of the occupation categorization from the 2000 Standard Occupation Classification (SOC) for illustrative purposes, with SOC major, SOC minor, SOC broad, SOC detailed and Census (2002) occupation codes displayed.

Table 2.4: Ever Worked For Pay Since Displacement

	(1)	(2)
Occupation	0.274	-0.122
Growth Rate	(0.199)	(0.273)
HS Diploma	0.0621***	0.0585***
	(0.0181)	(0.0179)
Some College	0.0798***	0.0629***
	(0.0169)	(0.0176)
BA/BS	0.143***	0.105***
	(0.0200)	(0.0204)
Graduate Degree	0.176***	0.110***
	(0.0330)	(0.0338)
Displaced 3	0.274***	0.282***
Years Ago	(0.0210)	(0.0198)
Displaced 2	0.0942*	0.0895*
Years Ago	(0.0526)	(0.0510)
Black	-0.0726***	-0.0601**
	(0.0245)	(0.0254)
Asian	-0.0467	-0.0431
	(0.0537)	(0.0526)
Other Race	-0.0611**	-0.0411
	(0.0287)	(0.0266)
Age 20-29	-0.00870	-0.00802
	(0.0197)	(0.0192)
Age 40-49	-0.0243	-0.0258
	(0.0223)	(0.0202)
Age 50 Plus	-0.153***	-0.155***
	(0.0201)	(0.0170)
Female	-0.0346	-0.0242
	(0.0220)	(0.0214)
Tenure	-0.000471	0.000288
	(0.00195)	(0.00186)
Tenure Squared	-0.000157**	-0.000174***
	(0.0000672)	(0.0000616)
State and Year FE	Yes	Yes
Occupation FE	No	Yes
Dept. var mean	0.707	0.707
Observations	5090	5090
Adjusted R2	0.134	0.153

Notes: Sample is limited to workers displaced because their plant or firm closed or moved, have a non-missing pre-displacement occupation state growth rate, were displaced from a full-time job, and did not move after displacement. The outcome variable is worked for pay since displacement. Education categories are indicator variables, with "Less Than HS" as the omitted category. Race categories are indicator variables, with "White" as the omitted category. Workers were displaced three, two or one year ago, with one year ago as the omitted category. Regression is a linear probability model with Displaced Workers Survey sample weights. All regressions have state and year of displacement fixed effects. Standard errors clustered by state. * p<0.1 ** p<0.05 *** p<0.01

Table 2.5: Log Duration of Joblessness

	(1)	(2)
Occupation	-4.459***	-3.859***
Growth Rate	(0.722)	(0.959)
HS Diploma	-0.0148	-0.0238
	(0.107)	(0.102)
Some College	0.195	0.220*
	(0.126)	(0.111)
BA/BS	0.0955	0.0786
	(0.110)	(0.118)
Graduate Degree	0.0827	0.00314
	(0.194)	(0.182)
Displaced 3	0.614***	0.596***
Years Ago	(0.0625)	(0.0634)
Displaced 2	0.572***	0.567***
Years Ago	(0.179)	(0.181)
Black	0.414***	0.390***
	(0.0664)	(0.0779)
Asian	0.0937	0.174
	(0.373)	(0.377)
Other Race	0.251*	0.254*
	(0.128)	(0.148)
Age 20-29	-0.118	-0.115
	(0.0799)	(0.0786)
Age 40-49	0.0869	0.0651
	(0.0707)	(0.0660)
Age 50 Plus	0.183**	0.162**
	(0.0781)	(0.0727)
Female	0.111	0.112
	(0.0715)	(0.0881)
Tenure	0.0216**	0.0217*
	(0.0103)	(0.0121)
Tenure Squared	-0.000339	-0.000388
	(0.000423)	(0.000469)
State and Year FE	Yes	Yes
Occupation FE	No	Yes
Dept. var mean	2.133	2.133
Observations	2902	2902
Adjusted R2	0.105	0.118

Notes: Sample is limited to workers displaced because their plant or firm closed or moved, have a non-missing pre-displacement occupation state growth rate, were displaced from a full-time job, and did not move after displacement. Workers must be reemployed after displacement. The outcome variable is log duration of joblessness, censored at 100 weeks. Education categories are indicator variables, with "Less Than HS" as the omitted category. Race categories are indicator variables, with "White" as the omitted category. Workers were displaced three, two or one year ago, with one year ago as the omitted category. All regressions use Displaced Workers Survey sample weights and have state and year of displacement fixed effects. Column (2) adds minor group occupation fixed effects. Standard errors are clustered by state. * p<0.1 ** p<0.05 *** p<0.01

Table 2.6: Occupation Change

	(1)	(2)
Occupation	-0.949***	-0.865***
Growth Rate	(0.211)	(0.209)
HS Diploma	0.0369	0.0228
	(0.0234)	(0.0201)
Some College	0.0700***	0.0442***
	(0.0233)	(0.0161)
BA/BS	0.0762*	0.0532
	(0.0390)	(0.0340)
Graduate Degree	0.0426	0.0743*
	(0.0392)	(0.0426)
Displaced 3	0.127***	0.121***
Years Ago	(0.0183)	(0.0171)
Displaced 2	0.0837*	0.0962**
Years Ago	(0.0441)	(0.0428)
Black	0.0342	0.0474*
	(0.0267)	(0.0252)
Asian	0.117***	0.111**
	(0.0432)	(0.0524)
Other Race	-0.0391	-0.0344
	(0.0260)	(0.0277)
Age 20-29	0.0459**	0.0537**
	(0.0191)	(0.0202)
Age 40-49	-0.00878	-0.0121
	(0.0178)	(0.0192)
Age 50 Plus	-0.00379	-0.00454
	(0.0207)	(0.0181)
Female	0.0350*	0.0102
	(0.0181)	(0.0196)
Tenure	0.00208	0.000906
	(0.00286)	(0.00313)
Tenure Squared	-0.000128	-0.000144
	(0.000105)	(0.000109)
State and Year FE	Yes	Yes
Occupation FE	No	Yes
Dept. var mean	0.640	0.640
Observations	4455	4455
Adjusted R2	0.0197	0.0833

Notes: Sample is limited to workers displaced because their plant or firm closed or moved, have a non-missing pre-displacement occupation state growth rate, were displaced from a full-time job, and did not move after displacement. Workers must be reemployed at the time of the survey and report a post-displacement occupation. The outcome variable is occupation change. Education categories are indicator variables, with "Less Than HS" as the omitted category. Race categories are indicator variables, with "White" as the omitted category. Workers were displaced three, two or one year ago, with one year ago as the omitted category. All regressions use Displaced Workers Survey sample weights and have state and year of displacement fixed effects. Column (2) adds minor group occupation fixed effects. Standard errors are clustered by state. * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Table 2.7: Change in Log Weekly Earnings

	(1)	(2)
Occupation	1.453**	2.203***
Growth Rate	(0.623)	(0.704)
HS Diploma	-0.0376	-0.0307
	(0.0414)	(0.0424)
Some College	-0.0555	-0.0305
	(0.0586)	(0.0576)
BA/BS	-0.120*	-0.0359
	(0.0668)	(0.0653)
Graduate Degree	-0.113	0.01000
	(0.101)	(0.117)
Displaced 3	-0.0115	-0.00320
Years Ago	(0.0553)	(0.0555)
Displaced 2	-0.0699	-0.0757
Years Ago	(0.122)	(0.129)
Black	-0.0937*	-0.150**
	(0.0551)	(0.0696)
Asian	0.0869	0.108
	(0.159)	(0.161)
Other Race	0.0109	0.00516
	(0.158)	(0.153)
Age 20-29	0.0635	0.0483
	(0.0645)	(0.0765)
Age 40-49	-0.0515	-0.0319
	(0.0565)	(0.0664)
Age 50 Plus	-0.120***	-0.127***
	(0.0414)	(0.0405)
Female	-0.0260	-0.0320
	(0.0411)	(0.0554)
Tenure	-0.00384	-0.00356
	(0.00695)	(0.00667)
Tenure Squared	-0.000118	-0.000150
	(0.000219)	(0.000213)
State and Year FE	Yes	Yes
Occupation FE	No	Yes
Dept. var mean	-0.237	-0.237
Observations	2737	2737
Adjusted R2	0.0161	0.0250

Notes: Sample is limited to workers displaced because their plant or firm closed or moved, have a non-missing pre-displacement occupation state growth rate, were displaced from a full-time job, and did not move after displacement. Workers must be reemployed at the time of the survey. The outcome variable is change in log earnings. Education categories are indicator variables, with "Less Than HS" as the omitted category. Race categories are indicator variables, with "White" as the omitted category. Workers were displaced three, two or one year ago, with one year ago as the omitted category. All regressions use Displaced Workers Survey sample weights and have state and year of displacement fixed effects. Column (2) adds minor group occupation fixed effects. Standard errors are clustered by state. Standard errors are clustered by state. * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Table 2.8: Comparison Between Occupation and Industry Growth Rates

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log Duration of Joblessness						
Occupation	-4.465***		-4.206***	-3.887***		-3.818***
Growth Rate	(0.725)		(0.675)	(0.962)		(0.853)
Industry		-2.113**	-0.409		-1.243	-0.130
Growth Rate		(0.808)	(0.769)		(0.871)	(0.806)
State and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	No	No	No	Yes	Yes	Yes
Dept. var mean	2.133	2.133	2.133	2.133	2.133	2.133
Observations	2881	2881	2881	2881	2881	2881
Adjusted R2	0.105	0.0985	0.105	0.118	0.114	0.118
Test of Equality	0.0001		0.00183	0.0002		0.00260
Panel B: Change in Log Earnings						
Occupation	1.509**		1.220	2.263***		2.121**
Growth Rate	(0.629)		(0.862)	(0.700)		(0.972)
Industry		0.936***	0.470		0.848*	0.269
Growth Rate		(0.314)	(0.498)		(0.472)	(0.655)
State and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	No	No	No	Yes	Yes	Yes
Dept. var mean	-0.237	-0.237	-0.237	-0.237	-0.237	-0.237
Observations	2723	2723	2723	2723	2723	2723
Adjusted R2	0.0162	0.0151	0.0161	0.0255	0.0222	0.0252
Test of Equality	0.4028		0.564	0.1378		0.242

Notes: Sample is limited to workers displaced because their plant or firm closed or moved, have a non-missing pre-displacement occupation state growth rate, were displaced from a full-time job, and did not move after displacement. Additionally, they must have a non-missing 3 digit NAICS state industry growth rate. Workers have worked for pay after displacement. Panel A's outcome is log unemployment duration and Panel B's outcome is change in log earnings. Panel B is limited to workers who were employed at the time of survey. Workers with fewer than $40 of earnings per week prior to displacement are excluded. Other controls included in all specifications are as follows: Education categories (indicator variables), race categories (indicator variables), age categories, and years since displacement. All regressions use Displaced Workers Survey sample weights and have state and year of displacement fixed effects. Columns (4)-(6) adds minor group occupation fixed effects. The test of equality row displays the p-value associated with the t-tests conducted comparing the occupation growth rate and industry growth rate. The p-value between Column (1) and (2) results from a test of equality of the occupation growth rate and industry growth rate coefficients in Columns (1) and (2). The p-value in Column (3) tests the hypothesis that the coefficients on occupation growth rate and industry growth rate in Column (3) are equal. Columns (4) - (6) follow the same pattern. Standard errors are clustered by state.* p<0.1 ** p<0.05 *** p<0.01

Table 2.9: Prior Year Occupation Growth Rate on Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Log Duration of Joblessness								
Contemporaneous	-4.061***				-3.714***			
	(0.881)				(1.125)			
Prior Year		-2.254**				-1.608		
		(1.023)				(1.123)		
Two Years Ago			-0.589				0.333	
			(1.255)				(1.224)	
Mean				-3.334***				-2.985**
				(1.208)				(1.412)
State and Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	No	No	No	No	Yes	Yes	Yes	Yes
Dept. var mean	2.073	2.073	2.073	2.073	2.073	2.073	2.073	2.073
Observations	2405	2405	2405	2405	2405	2405	2405	2405
Adjusted R2	0.0996	0.0935	0.0917	0.0950	0.116	0.111	0.110	0.112
Panel B: Change in Log Earnings								
Contemporaneous	1.579**				2.929***			
	(0.726)				(1.040)			
Prior Year		0.479				-1.206		
		(0.768)				(1.518)		
Two Years Ago			0.210				-2.254	
			(0.541)				(1.782)	
Mean				1.088				-0.710
				(0.954)				(2.018)
State and Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	No	No	No	No	Yes	Yes	Yes	Yes
Dept. var mean	-0.235	-0.235	-0.235	-0.235	-0.235	-0.235	-0.235	-0.235
Observations	2284	2284	2284	2284	2284	2284	2284	2284
Adjusted R2	0.0179	0.0155	0.0154	0.0162	0.0330	0.0271	0.0319	0.0264

Notes: Sample is limited to workers displaced from a full-time job because their plant or firm closed or moved, were displaced from a full-time job, did not move after displacement, and were re-employed after displacement. They must have a non-missing pre-displacement occupation state growth rate for the contemporaneous year, the year prior and two years prior. The sample, therefore, consists of workers displaced between 2003 and 2013. Mean occupation growth rate is the mean of the occupation growth rates of the contemporaneous year, the prior year, and two years ago. The outcome variable is log duration of joblessness, censored at 100 weeks. Workers with fewer than $40 of earnings per week prior to displacement are excluded. Other controls included in all specifications are as follows: Education categories (indicator variables), race categories (indicator variables), age categories, and years since displacement. All regressions use Displaced Workers Survey sample weights and have state and year of displacement fixed effects. Standard errors are clustered by state. * p<0.1 ** p<0.05 *** p<0.01

Table 2.10: Horse Race with Alternate Construction of Occupation Growth Rate

	(1)	(2)	(3)	(4)
	Log Duration of Joblessness		Change in Log Earnings	
Occupation Growth Rate	-5.590***	-5.085***	2.277	2.966
(Leaving Out Industry)	(1.428)	(1.668)	(1.788)	(1.803)
Industry Growth Rate	-1.559*	-1.166	0.711*	0.786
	(0.820)	(0.877)	(0.383)	(0.494)
State and Year FE	Yes	Yes	Yes	Yes
Occupation FE	No	Yes	No	Yes
Dept. var mean	2.133	2.133	-0.238	-0.238
Observations	2845	2845	2690	2690
Adjusted R2	0.105	0.118	0.0166	0.0255
Test of Equality	0.0216	0.0275	0.439	0.315

Notes: Sample is limited to workers displaced because their plant or firm closed or moved, have a non-missing pre-displacement occupation state growth rate, were displaced from a full-time job, and did not move after displacement. Additionally, they must have a non-missing 3 digit NAICS state industry growth rate. Workers have worked for pay after displacement. Workers with fewer than $40 of earnings per week prior to displacement are excluded. Other controls included in all specifications are as follows: Education categories (indicator variables), race categories (indicator variables), age categories, and years since displacement. The test of equality row displays the p-value associated with the t-tests conducted comparing the occupation growth rate and industry growth rate. All regressions use Displaced Workers Survey sample weights and have state and year of displacement fixed effects. * p<0.1 ** p<0.05 *** p<0.01

Table 2.11: Placebo Test- Occupation Growth Rate Four Years After Displacement on Log Duration of Joblessness

	(1)	(2)	(3)	(4)
Occupation Growth Rate				
Occupation Growth Rate in t	-4.422***		-3.207***	
	(0.814)		(1.004)	
Occupation Growth Rate in $t+4$		-2.297**		-0.425
		(1.003)		(1.183)
State and Year FE	Yes	Yes	Yes	Yes
Occupation FE	No	No	Yes	Yes
Dept. var mean	2.130	2.130	2.130	2.130
Observations	2354	2354	2354	2354
Adjusted R2	0.103	0.0934	0.118	0.114
Test of Equality	0.0688		0.1026	

Notes: Sample is limited to workers displaced from a full-time job because their plant or firm closed or moved, and have a non-missing pre-displacement occupation state growth rate, and have not moved after displacement. Workers must have worked for pay after displacement to be in the sample, which implies all durations are completed, and censored at 100 weeks. The sample is restricted to 2001-2010, to have the same sample for Columns (1) and (3) as (2) and (4). The test of equality displays the p-value associated with the t-test conducted comparing the (contemporaneous) occupation growth rate and the occupation growth rate four years later. Standard errors are clustered by state. * p<0.1 ** p<0.05 *** p<0.01

Table 2.12: Different Measures of Occupation Growth Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Nat'l Ind Growth Nat'l Occ-Ind Dist		Nat'l Ind Growth State Occ-Ind Dist		State Ind Growth Nat'l Occ-Ind Dist		State Ind Growth State Occ-Ind Dist	
Panel A: Log Duration of Joblessness								
Occupation	-4.462***	-3.862***	-2.795***	-2.537***	-1.947**	-1.416*	-1.263**	-0.986*
Growth Rate	(0.724)	(0.961)	(0.901)	(0.733)	(0.754)	(0.763)	(0.570)	(0.548)
State and Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	No	Yes	No	Yes	No	Yes	No	Yes
Dept. var mean	2.134	2.134	2.130	2.130	2.134	2.134	2.130	2.130
Observations	2890	2890	2871	2871	2890	2890	2871	2871
Adjusted R2	0.105	0.119	0.101	0.116	0.0992	0.115	0.0974	0.114
Panel B: Change in Log Earnings								
Occupation	1.457**	2.201***	0.911**	0.823**	0.422	0.626	0.266	0.122
Growth Rate	(0.624)	(0.705)	(0.401)	(0.359)	(0.358)	(0.475)	(0.256)	(0.212)
State and Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	No	Yes	No	Yes	No	Yes	No	Yes
Dept. var mean	-0.237	-0.237	-0.236	-0.236	-0.237	-0.237	-0.236	-0.236
Observations	2728	2728	2713	2713	2728	2728	2713	2713
Adjusted R2	0.0160	0.0249	0.0144	0.0214	0.0139	0.0215	0.0134	0.0205

Notes: Sample is limited to workers displaced from a full-time job because their plant or firm closed or moved, and have a non-missing pre-displacement occupation state growth rate, and have not moved after displacement. The occupation growth rate is constructed in the following four ways: Columns (1) and (2) as measured throughout the paper, Columns (3) and (4) replacing national occupation by industry composition at the beginning with state-specific occupation by industry composition for 2012, Columns (5) and (6) replacing national industry growth with state-specific industry growth, and Columns (7) and (8) making both changes. Additional details are specified in the text. The outcome variable in Panel (A) is log duration of joblessness and the outcome variable in Panel (B) is change in log earnings. Workers must be reemployed after displacement. Workers with fewer than $40 of earnings per week prior to displacement are excluded. Other controls included in all specifications are as follows: Education categories (indicator variables), race categories (indicator variables), age categories, and years since displacement. All regressions use Displaced Workers Survey sample weights and have state and year of displacement fixed effects. The even columns add minor group occupation fixed effects. Standard errors are clustered by state. * p<0.1 ** p<0.05 *** p<0.01

Table 2.13: Log Duration of Joblessness - Censored Regression

	(1)	(2)
Occupation Growth Rate	-3.878***	-1.998
	(1.037)	(1.261)
Dept. var mean	2.133	2.133
Observations	4530	4530
State and Year FE	Yes	Yes
Occupation FE	No	Yes

Notes: Sample is limited to workers displaced from a full-time job because their plant or firm closed or moved, and have a non-missing pre-displacement occupation state growth rate. Sample is limited to workers displaced from a full-time job because their plant or firm closed or moved, and have a non-missing pre-displacement occupation state growth rate, and have not moved after displacement. For workers who are not reemployed, the minimum jobless duration is 26 weeks if they were displaced the year prior to the survey, 78 weeks if they were displaced two years prior to the survey, and 130 weeks if they were displaced three years prior to the survey. Columns (1) and (2) are specifications with and without minor group occupation fixed effects. Workers with fewer than $40 of earnings per week prior to displacement are excluded. Other controls included in all specifications are as follows: Education categories (indicator variables), race categories (indicator variables), age categories, and years since displacement. All regressions use Displaced Workers Survey sample weights and have state and year of displacement fixed effects. Standard errors are clustered by state. * p<0.1 ** p<0.05 *** p<0.01

Table 2.14: Sensitivity to Functional Forms

	(1)	(2)	(3)	(4)	(5)	(6)
	Duration of Joblessness (in weeks)		Exhausted Unemployment Benefits		More Than 26 Weeks of Joblessness	
Occupation Growth Rate	-73.53*** (13.57)	-63.71*** (17.77)	-0.440 (0.266)	-0.748* (0.399)	-0.777** (0.319)	-1.245*** (0.232)
State and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	No	Yes	No	Yes	Yes	No
Dept. var mean	15.15	15.15	0.387	0.387	0.330	0.330
Observations	3439	3439	2550	2550	4093	4093
Adjusted R2	0.106	0.120	0.0883	0.106	0.183	0.170
	Δ Weekly Earnings		Δ Weekly Earnings Inc Not Re-Employed		Δ Weekly Earnings Inc Not Re-Employed Excluding Outliers	
Occupation Growth Rate	623.6** (292.1)	1153.2*** (387.4)	590.9** (268.9)	598.5 (359.0)	614.3*** (220.5)	555.5** (256.9)
State and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	No	Yes	No	Yes	No	Yes
Dept. var mean	-98.48	-98.48	-286.3	-286.3	-280.6	-280.6
Observations	2737	2737	3956	3956	3878	3878
Adjusted R2	0.0384	0.0557	0.107	0.124	0.129	0.149

Notes: Sample is limited to workers displaced from a full-time job because their plant or firm closed or moved, and have a non-missing pre-displacement occupation state growth rate, and have not moved after displacement. Workers must be reemployed after displacement. Workers with fewer than $40 of earnings per week prior to displacement are excluded. In the top panel, Columns (1) and (2) are levels of duration of joblessness (in weeks). Columns (3) and (4) are a binary indicator for having exhausted formal unemployment benefits. Columns (5) and (6) are an indicator for more than 26 weeks of joblessness. In the bottom panel, Columns (1) and (2) the outcome is change in weekly earnings (in levels). Columns (3) and (4) include the workers who have not been re-employed since displacement, and consider their entire lost job earnings as the change in weekly earnings. Columns (5) and (6) add to this Column (3) and (4) a restriction of excluding workers at the bottom 1st percentile or top 99th percentile of the change in weekly earnings distribution. All regressions include controls: education categories are indicator variables, with "Less Than HS" as the omitted category. Race categories are indicator variables, with "White" as the omitted category. Workers were displaced three, two or one year ago, with one year ago as the omitted category. All regressions use Displaced Workers Survey sample weights and have state and year of displacement fixed effects. Standard errors are clustered by state. * p<0.1 ** p<0.05 *** p<0.01

Chapter 3

The Impact of Paid Family Leave Benefits: Regression Kink Evidence from California Administrative Data

3.1 Introduction

A vast body of research has documented a persistent "motherhood wage penalty" that can last 10 to 20 years after childbirth. Mothers earn lower wages, work fewer hours, and are less likely to be employed than fathers or childless women and men (see, e.g.: Waldfogel, 1998; Lundberg and Rose, 2000; Blau and Kahn, 2000; Anderson et al., 2002; Molina and Montuenga, 2009; Angelov et al., 2016; Chung et al., 2017; Kleven et al., 2018, 2019), and these differences are particularly pronounced for highly-educated women at the top of the female earnings distribution (Anderson et al., 2002; Bertrand et al., 2010; Hotchkiss et al., 2017; Chung et al., 2017). Paid family leave (PFL)—a policy that allows working mothers to take time off work to recover from childbirth and care for their newborn (or newly adopted) children while receiving partial wage replacement—

may be a tool for reducing this penalty if it facilitates career continuity and advancement for women. However, opponents of PFL caution that it could have the opposite effect: by allowing mothers to have paid time away from work, PFL may lower their future labor market attachment, while employers could face substantial costs that lead to increased discrimination against women.[1] These discussions are especially fervent in the United States, which is the only developed country without a national paid maternity or family leave policy.

In this paper, we use administrative data from California—the first state to implement a PFL program (hereafter, CA-PFL)—and use a regression kink (RK) design to identify the effects of the benefit amount on leave duration, labor market outcomes, and subsequent leave-taking among high-earning mothers.[2] Isolating the effect of the benefit amount is critical for informing debates about payment during leave. Since the vast majority of American workers already have access to unpaid leave through their employers and the federal Family and Medical Leave Act (FMLA), the wage replacement rate is arguably the most salient parameter under debate.[3] A long literature on other social insurance programs—including unemployment insurance (UI) (Baily, 1978; Chetty, 2008; Card et al., 2012; Landais, 2015; Card et al., 2015a,b, 2016b; Schmieder and Von Wachter, 2016; Schmieder and von Wachter, 2017), Social Security Disability Insurance (SSDI) (Gelber et al., 2016), and the Workers' Compensation program (Hansen et al., 2017)—finds a

[1]For more information on the arguments surrounding paid leave in the U.S., see, e.g.: `https://www.usnews.com/news/best-states/articles/2017-04-07/affordable-child-care-paid-family-leave-key-to-closing-gender-wage-gap` and `https://economix.blogs.nytimes.com/2014/01/27/the-business-of-paid-family-leave/?_r=0`.

[2]As we detail in Section 3.2, most women in California are eligible for a total of up to 16 weeks of paid leave.

[3]Data from the 2016 National Compensation Survey show that 88 percent of civilian workers have access to unpaid leave through their employers (see: `https://www.bls.gov/ncs/ebs/benefits/2016/ownership/civilian/table32a.htm`. The FMLA was enacted in 1993 and provides 12 weeks of *unpaid* job protected family leave to qualifying workers. To be eligible for the FMLA, workers must have worked at least 1,250 hours in the preceding year for an employer with at least 50 employees (within a 75 mile radius of the employment location). According to most recent data from 2012, about 60 percent of American private sector workers are eligible for the FMLA (Klerman et al., 2012).

positive relationship between the benefit amount and program participation duration, with elasticities ranging between 0.3 and 2 in the case of UI (Card et al., 2015a).[4] As such, a higher PFL benefit may increase maternity leave duration, which could in turn adversely affect women's subsequent labor market trajectories.[5]

Since the leave benefit amount is not randomly assigned, it is challenging to disentangle its causal impact from the possible influences of other unobservable differences between individuals. To circumvent this issue, we make use of a kink in the PFL benefit schedule in California: during our analysis time frame, participants get 55 percent of their prior earnings replaced, up to a maximum benefit amount.[6] Intuitively, we compare the outcomes of mothers with pre-leave earnings just below and just above the threshold at which the maximum benefit applies. These women have similar observable characteristics, but face dramatically different marginal wage replacement rates of 55 and 0 percent, respectively. The RK method identifies the causal effect of the benefit amount by testing for a change in the slope of the relationship between an outcome and pre-claim earnings at the same threshold (Card et al., 2016b).

While a key advantage of the RK method is that it can account for the endogeneity in the benefit amount, the primary limitation is that the RK sample is not representative of the population of leave-takers. The kink is located around the 92nd percentile of the California female earnings distribution, and women in the vicinity of the kink point are older and work in larger firms than the average female program participant. That

[4]A recent paper on the elasticity of injury leave duration with respect to the benefit amount provided under Oregon's Workers' Compensation program finds an elasticity estimate in the range of 0.2 to 0.4 (Hansen et al., 2017).

[5]If higher benefits increase maternity leave duration, the impacts on women's future labor market outcomes are theoretically ambiguous (Klerman and Leibowitz, 1994; Olivetti and Petrongolo, 2017a). Increased time away from the job may be detrimental to future labor market success as a result of human capital depreciation or employer discrimination. Alternatively, if a higher benefit encourages a longer leave for a mother who would have otherwise quit her job, then there may be a positive effect on her future labor market outcomes through increased job continuity.

[6]More details on the program are in Section 3.2.

being said, high-earning women's careers may be especially sensitive to employment interruptions—for example, Stearns (2016) shows that access to job protected paid maternity leave in Great Britain reduces the likelihood that high-skilled women are promoted or hold management positions five years after childbirth. In the U.S., Hotchkiss et al. (2017) document that the motherhood penalty for college graduates is approximately double that of women with only a high school degree.

Additionally, RK estimates provide information about the implications of benefit changes around the maximum benefit threshold. These are highly policy relevant because all existing state PFL programs, as well as the current national PFL proposal (the Family and Medical Insurance Leave Act, or FAMILY Act), feature similar kinked benefit schedules, but have different kink point locations.[7]

Our results show that higher benefits do *not* increase maternity leave duration among women with earnings near the maximum benefit threshold. Our RK estimates allow us to rule out that a 10 percent increase in the weekly benefit amount (WBA) would increase leave duration by more than 0.3 to 2.1 percent (i.e., we can reject elasticities higher than 0.03 to 0.21), depending on the specification. Our results underscore the notion that PFL provides a distinct type of social insurance and targets a unique population of parents and caregivers, making the (much larger) elasticities from the prior social insurance literature less relevant for PFL (Krueger and Meyer, 2002).

We also find no evidence that PFL benefits have any adverse consequences on subsequent

[7]The states with PFL policies are: California (since 2004), New Jersey (since 2008), Rhode Island (since 2014), New York (since 2018), Washington state (will go into effect in 2020), Washington D.C. (will go into effect in 2020), and Massachusetts (will go into effect in 2021). In all states, benefits are paid as a percentage of prior earnings, up to a maximum benefit amount. The wage replacement rates are: 55 percent (California), 66 percent (New Jersey), 60 percent (Rhode Island), 67 percent (New York). D.C.'s marginal replacement rates vary with prior earnings. The maximum weekly benefit amounts as of 2018 are: $1,216 (California), $637 (New Jersey), $831 (Rhode Island), and $652.86 (New York). More information is available here: `https://fas.org/sgp/crs/misc/R44835.pdf`. For information on the FAMILY Act, see: `http://www.nationalpartnership.org/research-library/work-family/paid-leave/family-act-fact-sheet.pdf`.

maternal labor market outcomes for high-earning women in our sample. A higher benefit amount does not have a significant effect on the likelihood of returning to employment following the end of the leave. However, conditional on returning to work, we find that women who receive a higher benefit during leave are more likely to return to their pre-leave employers rather than find new jobs: a 10 percent increase in the WBA raises the likelihood of return to the pre-leave firm (conditional on any employment) by 0.3 to 4.2 percentage points (0.3 to 5 percent), depending on specification. While our data do not allow us to observe the exact mechanisms underlying this result, it is possible that higher benefits during leave improve worker morale or promote firm loyalty (even if she recognizes that her employer is not paying her benefits directly), similar in spirit to efficiency wage models (Akerlof, 1984; Stiglitz, 1986; Katz, 1986; Krueger and Summers, 1988).[8]

Lastly, we provide novel evidence that the benefit amount predicts repeat program use. We find that an additional 10 percent in the benefit received during a mother's first period of leave is associated with a 0.8 to 1.6 percentage point higher likelihood of having another PFL claim within the following three years (a 3 to 7 percent increase), depending on the specification. This effect may in part operate through the positive impact on the likelihood of return to the pre-leave employer after the first period of leave. As shown in Bana et al. (2018d), firm-specific factors (potentially including workplace culture and information provision) explain a substantial amount of the variation in CA-PFL take-up. Our results suggest that a higher benefit amount causes mothers to return to the firms where they took their first period of leave instead of switching to different firms, which could have lower leave-taking rates. It is also possible that women who get more wage replacement during leave may simply have a better experience and are therefore

[8]By contrast, our results are inconsistent with prior evidence of an income effect that reduces employment: Wingender and LaLumia (2017) find that higher after-tax income during a child's first year of life reduces labor supply among new mothers.

more likely to participate in the program again than those with lower benefits. Indeed, a similar relationship between current benefits and future claims has been found in the context of the Workers' Compensation program in Oregon (Hansen et al., 2017).[9]

Our study builds on several recent papers that use survey data to analyze the labor market effects of CA-PFL with difference-in-difference (DD) designs (Rossin-Slater et al., 2013a; Bartel et al., 2018a; Das and Polachek, 2015a; Baum and Ruhm, 2016a; Stanczyk, 2016; Byker, 2016). Our analysis of administrative data can overcome several limitations of these studies, which include small sample sizes, measurement error, non-response bias, lack of panel data, and missing information on key variables such as PFL take-up and leave duration.[10]

We also contribute to a body of research set outside the U.S., in which studies have analyzed the impacts of extensions in existing PFL policies (or, less frequently, introductions of new programs) on maternal leave-taking and labor market outcomes, delivering mixed results (see Olivetti and Petrongolo, 2017a and Rossin-Slater, 2018a for recent overviews).[11] The substantial cross-country heterogeneity in major policy

[9]It is also possible that the increase in repeat leave-taking arises due to a change in fertility behavior, although past research offers mixed evidence on the relationship between PFL and fertility. For example, Dahl et al. (2016) find no effects of Norwegian maternity leave extensions on mothers' completed fertility. By contrast, Lalive and Zweimüller (2009) find that an extension in parental leave in Austria increased subsequent fertility rates among mothers. In the case of CA-PFL, Lichtman-Sadot (2014) finds some evidence that disadvantaged women re-timed their pregnancies to become eligible for CA-PFL in the second half of 2004. However, we are not aware of any studies documenting effects of CA-PFL on subsequent fertility.

[10]In an ongoing study, Campbell et al. (2017) use administrative data from Rhode Island to study the effects of paid maternity leave provided through Rhode Island's Temporary Disability Insurance system on maternal and child outcomes, exploiting the earnings threshold for TDI eligibility. Our focus on high-earning women in California is complementary to their evidence on women at the low end of the earnings distribution.

[11]For example, some studies find either positive or zero effects on maternal employment in the years after childbirth (Baker and Milligan, 2008; Kluve et al., 2013; Bergemann and Riphahn, 2015; Carneiro et al., 2015; Dahl et al., 2016; Stearns, 2016), while others document negative impacts, especially in the long-term (Lalive and Zweimüller, 2009; Lequien, 2012; Schönberg and Ludsteck, 2014; Bičáková and Kalíšková, 2016; Canaan, 2017). Cross-country comparisons suggest that provisions of leave up to one year in length typically increase the likelihood of employment shortly after childbirth, whereas longer leave entitlements can negatively affect women's long-term labor market outcomes (Ruhm, 1998; Blau and Kahn, 2013; Thévenon and Solaz, 2013; Olivetti and Petrongolo, 2017a).

components—the benefit amount, statutory leave duration, and job protection—generates challenges for comparing policies and likely contributes to the lack of consistency in the literature.[12]

Additionally, we bring the novel RK research design to isolate the effect of the PFL benefit amount.[13] To the best of our knowledge, the only existing study that isolates the effect of the maternity leave wage replacement rate while holding constant other policy parameters is set in Japan and finds no impact on maternal job continuity or leave duration (Asai, 2015a).[14] This evidence may not be readily applicable to the U.S. setting, however, since Japanese mothers are guaranteed one year of job protected paid maternity leave. By contrast, U.S. maternity leave durations are much shorter and often not job protected, and even among the highest-wage workers, less than a quarter have access to *any* employer-provided paid leave.[15]

The rest of the paper unfolds as follows. Section 3.2 provides more details on the CA-PFL program and the benefit schedule. Section 3.3 describes our data, while Section 3.4 explains our empirical methods. Section 3.5 presents our results and sensitivity analyses, while Section 3.6 offers some conclusions.

[12]See Addati et al. (2014) and Olivetti and Petrongolo (2017a) for more information on maternity and family leave policy details in countries around the world.

[13]Less relevant to the topic of this paper, the RK research design has also been used in studies of student financial aid and higher education (Nielsen et al., 2010; Turner, 2014; Bulman and Hoxby, 2015), tax behavior (Engström et al., 2015; Seim, Forthcoming), payday lending (Dobbie and Skiba, 2013), and local government expenditures (Garmann, 2014; Lundqvist et al., 2014).

[14]We are also aware of two other studies that isolate the impacts of other PFL policy parameters in countries outside the U.S.: Lalive et al. (2014) separately estimate the labor market impacts of the duration of paid leave and job protection for Austrian mothers, while Stearns (2016) distinguishes between access to any paid leave and job protection in Great Britain.

[15]Data from the 2016 National Compensation Survey show that 14 percent of all civilian workers have access to PFL through their employers. Among those in occupations with wages in the highest decile, 23 percent have access to employer-provided PFL. With regard to leave duration, Rossin-Slater et al. (2013a) estimate that California mothers took an average of about three weeks of maternity leave prior to the implementation of CA-PFL.

3.2 Background on CA-PFL and the Benefit Schedule

Californian mothers have been eligible for several weeks of paid maternity leave to prepare for and recover from childbirth through California's State Disability Insurance (CA-SDI) system since the passage of the 1978 Pregnancy Discrimination Act. In 2004, most working mothers also became eligible for 6 weeks of leave through CA-PFL, which they can take anytime during the child's first year of life.[16] In total, women with uncomplicated vaginal deliveries can get up to 16 weeks of paid maternity/family leave through SDI and PFL.[17] Paid leaves under SDI and PFL are not directly job protected, although job protection is available if the job absence simultaneously qualifies under the federal Family and Medical Leave Act (FMLA) or California's Family Rights Act (CFRA).[18]

The CA-PFL/SDI benefit schedule is a piece-wise linear function of base period earnings, which is defined as the maximum quarterly earnings in quarters 2 through 5 before the claim. Figures 3.2a and 3.2b plot the WBA as a function of quarterly based period earnings in nominal terms for the years 2005 and 2014, the first and last years in our data, respectively. These graphs clearly show that there is a kink in the relationship between the WBA and base period quarterly earnings—the slope of the

[16]To be eligible for CA-SDI and CA-PFL, an individual must have earned at least $300 in wages in a base period between 5 and 18 months before the PFL claim begins. Only wages subject to the SDI tax are considered in the $300 minimum. California's PFL and SDI programs are financed entirely through payroll taxes levied on employees.

[17]Women who have a vaginal delivery can get up to four weeks of leave before the expected delivery date and up to six weeks of leave after the actual delivery date through CA-SDI. A woman's doctor may certify for her to obtain a longer period of SDI leave if the delivery is by Cesarean section, or if there are medical complications that prohibit her from performing her regular job duties.

[18]The CFRA is nearly identical to the FMLA in its provisions and eligibility criteria. There are minor differences between the two laws: for example, women who have difficult pregnancies can use FMLA prior to giving birth, but CFRA leave can only be used after childbirth. See: `https://www.shrm.org/resourcesandtools/tools-and-samples/hr-qa/pages/californiadifferencecfrafmla.aspx`.

benefit schedule changes from $\frac{0.55}{13} = 0.04$ to 0 at the maximum earnings threshold. Note that the replacement rate is divided by 13 to convert to a weekly amount since there are 13 weeks in a quarter. The location of this kink varies over time (i.e., both the maximum benefit amount and the earnings threshold change).[19] These graphs highlight that individuals with earnings near the kink point—who form the basis for our RK estimation—are relatively high earners. We describe the characteristics of our analysis sample in more detail in Section 3.3 below.

Finally, although the state pays PFL and SDI benefits according to the schedule just described, individual employers are able to supplement these benefits, making it possible for an employee to receive up to 100 percent of her base period earnings. To the extent that this phenomenon occurs, it diminishes the strength of the first stage relationship in our analysis, since some employees effectively do not face a kinked benefit schedule. While we could find no anecdotal evidence suggesting that this practice is common, we also have no data on such supplemental payments, and are therefore unable to precisely assess the magnitude of any attenuation. We can, however, focus on sub-samples of the data where this issue is least likely to be important: employees who made claims soon after the implementation of CA-PFL (2005-2010), employees who are *not* in the information/technology industry, and employees at firms with fewer than 1,000 workers. In all three cases, the pattern of findings remains the same, although the estimates are less precise (see Section 3.5 for more details).

[19] The nominal quarterly earnings thresholds for 2005 and 2014 were $19,830 and $25,385, respectively. In $2014 dollars, the 2005 threshold is $23,461.09. Figure 3.2c plots the maximum WBA in nominal terms in each quarter during our sample time frame. The maximum WBA has nominally increased from $840 in 2005 to $1,075 in 2014. In $2014 dollars, this translates to an increase from $1,018.22 to $1,075 during this time period.

3.3 Data and Sample

We use two administrative data sets available to us through an agreement with the California Employment Development Department (EDD).

First, we have data on the universe of PFL claims from 2005 to 2014. For each claim, we have information on the reason for the claim (bonding with a new child or caring for an ill family member), claim effective date, claim filed date, the total benefit amount received, the authorized weekly benefit amount, the employee's date of birth, the employee's gender, and a unique employee identifier.[20] For women, we also have an indicator for whether there was an associated transitional SDI claim (i.e., an SDI claim for the purposes of preparation for and recovery from childbirth), along with the same information for SDI claims as we do for PFL claims.

Second, we have quarterly earnings data over 2000-2014 for the universe of employees working for an employer that reports to the EDD tax branch.[21] For each employee, we have her unique identifier, her earnings in each quarter and in each job, a unique employer identifier associated with those earnings, and a North American Industry Classification System (NAICS) industry code associated with that employer.

Sample construction and key variables. For our main analysis sample, we begin with the universe of female PFL claims for the purpose of bonding with a new child (hereafter, "bonding claims" or "bonding leave") over 2005-2014.[22] We then merge the claims data to the quarterly earnings data using employee identifiers, and limit our sample

[20]The employee identifiers in our data are scrambled. Thus, we cannot actually identify any individual in our data set, but we can link information across data sets for each employee using the unique identifiers.

[21]Employers that employ one or more employees and pay wages in excess of $100 in a calendar quarter are required to report to the EDD according to California law. See `http://www.edd.ca.gov/pdf_pub_ctr/de44.pdf`.

[22]In previous versions of this paper, we had also reported results for male bonding claimants. However, since there are substantially fewer men than women in our claims data, the RK analysis yields imprecise results for fathers, and we have opted to focus our current analysis on mothers.

to the *first* bonding claim observed for each woman.[23] Next, since the location of the kink changes over our sample time frame (recall Figure 3.1), we drop women who make their first bonding claim in quarters during which these changes happen.[24]

For each claim, we assign the relevant base period earnings by calculating the maximum quarterly earnings (summing over all earnings each quarter for workers holding multiple jobs) in quarters 2 through 5 before the claim effective date. We also obtain information on the size and industry code associated with the most recent employer prior to the claim. For workers who have multiple jobs, we use the employer associated with the highest earnings. Employer size is calculated by adding up all of the employees working at that firm in that quarter.

Next, in an effort to create a sample that is reasonably homogeneous and most likely to be affected by the kink variation, we make the following sample restrictions: (1) We only include women who are aged 20-44 at the time of the first bonding claim; (2) We only keep female workers with base period quarterly earnings within a $\$10,000$ bandwidth of the kink point; (3) We drop women employed in industries in which employees are least likely to be subject to the SDI tax—private household workers, elementary and secondary school teachers, and public administration.

We then create a variable measuring the duration of leave in weeks by dividing the total benefit amount received by the authorized WBA. Since PFL does not need to be taken continuously, this duration measure accounts for possible gaps in between periods of leave. For women who make both bonding and transitional SDI claims, we add the

[23]Note that the first bonding claim may not necessarily be for the firstborn child. Some mothers may have chosen not to claim PFL for their firstborn child (but do claim for a later-born). Additionally, many mothers had lower parity children before CA-PFL existed. Unfortunately, we cannot link our EDD data to information on births, and we therefore cannot focus on claims for firstborns only.

[24]We do so because we observe that in these quarters some individuals get assigned their WBA according to the old schedule, while others according to the new schedule. Women with first bonding claims in the following quarters are dropped: 2005q1, 2007q4, 2009q1, 2010q1, 2012q1, 2013q1, and 2014q1.

two durations.[25] We analyze the natural log of leave duration in all of our specifications.

In addition to studying leave duration, we examine several post-leave labor market outcomes. We create indicators for being employed in the two, three, and four quarters after the quarter of the initiation of the claim (as measured by having any earnings in those quarters). We also create indicators for working at the pre-leave employer in quarters two, three, and four post-claim, which take the value 1 for mothers whose highest earnings in those quarters come from their pre-claim firms and 0 otherwise. We create these indicators separately conditioning and not conditioning on any employment in the respective quarters. We also calculate the change in the log of total earnings (in $2014) in quarters 2 through 5 post-claim relative to quarters 2 through 5 pre-claim. Lastly, we create an indicator for any subsequent PFL bonding claim in the 12 quarters after the first bonding claim.

Summary statistics. Table 3.1 presents the means of key variables for women in the $10,000 bandwidth sample, as well as for women in narrower ($2,500, $5,000, and $7,500) bandwidths of base period quarterly earnings surrounding the kink point. As we zoom in closer to the threshold, women in our sample become slightly older, work in somewhat larger firms, and have higher base period earnings.

For descriptive ease, the following discussion focuses on the $5,000 bandwidth sample. About 32 percent of the women are employed in the health industry before the claim, which is the top female industry in our data. The average weekly benefit received is $933 (in $2014), while average leave duration is almost 12 weeks, which is consistent with most women filing both transitional SDI and PFL bonding claims. When we consider subsequent labor market outcomes, we see that on average, 87, 86, and 85 percent of women are employed in quarters two, three, and four post-claim, respectively.

[25] We cap the maximum combined duration on SDI and PFL at 24 weeks (the 99th percentile).

Conditional on any employment, 88, 83, and 80 percent of women are employed by their pre-leave firms in these quarters, respectively. We also see that women have 10 percent lower earnings post-claim than they did pre-claim. Lastly, 23 percent of women make a subsequent bonding claim in the next three years.

Lastly, to provide more information on characteristics of women included in our analysis sample that are not available in the EDD data, we use data from the 2005-2014 American Communities Survey (ACS) on comparable Californian mothers of children under age 1.[26] We use each woman's prior year earnings to calculate her average quarterly earnings (by dividing by four), and then use them to find her place in the prior year's benefit schedule.[27] Appendix Table A.1 reports means of characteristics of women in the same bandwidths as in Table 3.1. In the \$5,000 bandwidth sample, 48 percent of mothers are non-Hispanic white, 4 percent are non-Hispanic black, while 12 percent are Hispanic. The vast majority of these women—91 percent—are married, and average spousal annual earnings (including zeros for women who are not married) are \$90,712 (in \$2014).

3.4 Empirical Design

We are interested in identifying the causal impacts of PFL/SDI benefits on mothers' leave duration, labor market outcomes, and subsequent claiming. To make our research question more precise, consider the following stylized model:

$$Y_{iq} = \gamma_0 + \gamma_1 ln(b_{iq}) + u_{iq} \tag{3.1}$$

[26]For comparability with the EDD data, we make similar restrictions to the ACS sample: (1) We only include women who are aged 20-44; (2) We drop women employed in industries in which employees are least likely to be subject to the SDI tax—private household workers, elementary and secondary school teachers, and public administration; (3) We drop women with zero reported earnings in the previous year.

[27]This procedure generates measurement error in assigning women to the benefit schedule, which, as we explain above, uses women's *maximum* (not average) quarterly earnings in quarters 2 through 5 before the claim. Unfortunately, we do not have information on quarterly earnings in the ACS.

for each woman i who makes a benefit claim in year by quarter (year×quarter) q.[28] Y_{iq} is an outcome of interest, such as log leave duration or an indicator for returning to the pre-leave firm. $ln(b_{iq})$ is the natural log of the WBA (in \$2014), while u_{iq} is a random vector of unobservable individual characteristics. We are interested in estimating γ_1, which measures the effect of a 100 percent increase in the WBA on the outcome of interest. The challenge with estimating equation (3.1) using an ordinary least squares (OLS) regression is that there are unobserved variables that are correlated with the benefit amount that may also affect our outcomes of interest, making it difficult to separate out the causal effect of the benefit from the influences of these other factors.

To overcome this challenge, we leverage quasi-experimental variation stemming from a kink in the CA-PFL/SDI benefit schedule. The benefit function can be described as follows: For each individual i who files a claim in quarter q, $b_{iq}(E_i, b_q^{max}, E_q^0)$ is a fixed proportion, $\tau = \frac{0.55}{13} = 0.04$, of an individual's base period earnings, E_i, up to the maximum benefit in quarter q, b_q^{max}, where E_q^0 denotes the earnings threshold that corresponds to the amount of base period earnings above which all employees receive the maximum benefit amount:

$$b_{iq}(E_i, b_q^{max}, E_q^0) = \begin{cases} \tau \cdot E_i & \\ b_q^{max} & \text{if} \quad E_i \geq E_q^0 \end{cases}$$

Put differently, there is a negative change in the slope of $b_{iq}(\cdot)$ at the earnings threshold, E_q^0, from 0.04 to 0. The RK design, described in detail by Card et al. (2012), Card et al. (2015b) and Card et al. (2016b), makes use of this change in the slope of the benefit function to estimate the causal effect of the benefit amount on the outcome of interest. Intuitively, the RK method tests for a change in the slope of the relationship between

[28]Throughout the paper, we use the terms "year×quarter" and "quarter" interchangeably. We are referring to each distinct quarter over our analysis time frame, i.e., 2005q1 through 2014q4.

the outcome and base period earnings at the earnings threshold. Assuming that—in the absence of the kink in the benefit function—there would be a smooth (i.e., non-kinked) relationship between the outcome and base period earnings, evidence of a change in the slope would imply a causal effect of the benefit amount on the outcome. The RK design can be thought of as an extension of the widely used Regression Discontinuity (RD) method, and Card et al. (2016b) provide a guide for practitioners on how local polynomial methods for estimation and inference (Porter, 2003; Imbens and Lemieux, 2008; Imbens and Kalyanaraman, 2012; Calonico et al., 2014, 2016) can be applied to the RK setting.

More formally, the RK estimator identifies:

$$\gamma_{RK} = \frac{lim_{\epsilon \uparrow 0}\left[\frac{\partial Y|E=E_q^0+\epsilon}{\partial E}\right] - lim_{\epsilon \downarrow 0}\left[\frac{\partial Y|E=E_q^0+\epsilon}{\partial E}\right]}{lim_{\epsilon \uparrow 0}\left[\frac{\partial ln(b)|E=E_q^0+\epsilon}{\partial E}\right] - lim_{\epsilon \downarrow 0}\left[\frac{\partial ln(b)|E=E_q^0+\epsilon}{\partial E}\right]} \tag{3.2}$$

In words, the RK estimator is a ratio of two terms. The numerator is the change in the slope of the outcome as a function of base period earnings at the earnings threshold. The denominator is the change in the slope of the benefit function at the earnings threshold.

In theory, if benefit assignments followed the formula exactly and our data contained no measurement errors, then the denominator in the ratio in equation (3.2) would be a known constant. In practice, as in many other policy settings, there may be small deviations from the benefit formula due to non-compliance or measurement error. Additionally, in our setting, only base period earnings *subject to the SDI tax* are used to calculate SDI and PFL benefits, but we cannot distinguish between earnings that are and are not subject to this tax in our data. As such, we must estimate the slope change in the denominator of equation (3.2) in a "fuzzy" RK design.[29]

[29] The "fuzzy" RK design is formally discussed in detail in Card et al. (2015b).

For estimation, we follow the methods outlined in Card et al. (2015b) and Card et al. (2016b). In particular, the slope changes in the numerator and denominator in equation (3.2) are estimated with local polynomial regressions to the left and right of the kink point. Key to this estimation problem are choices about the kernel, the bandwidth, and the order of the polynomial. We follow the literature by using a uniform kernel, which allows us to apply a simple two-stage least squares (2SLS) method (i.e., the denominator is estimated with a first stage regression).[30]

There is an active econometrics literature on optimal bandwidth choice in RD and RK settings. For all of our outcomes, we first present estimates using all possible bandwidths in \$500 increments from \$2,500 to \$10,000 of quarterly earnings. Additionally, we implement three different algorithms proposed in the literature: a version of the Imbens and Kalyanaraman (2012) bandwidth for the fuzzy RK design (hereafter, "fuzzy IK"),[31] as well as a bandwidth selection procedure developed by Calonico et al. (2014) (hereafter, "CCT") with and without a bias-correction ("regularization") term.[32] Moreover, following other RK studies, we try local linear and quadratic polynomials.

We estimate the following first stage regression:

$$ln(b_{iqw}) = \beta_0 + \sum_{p=1}^{\bar{p}} [\psi_p (E_i - E_q^0)^p + \theta_p (E_i - E_q^0)^p \cdot D_i] + \omega_q + \alpha_w + \rho' X_i + e_{iqw} \quad \text{if} \quad |E_i - E_q^0| \leq h \tag{3.3}$$

for each woman i with a first bonding claim in year×quarter q that was initiated in week of quarter w and with base period earnings E_i in a narrow bandwidth h surrounding the

[30]Card et al. (2016b) note that while a triangular kernel is boundary optimal, the efficiency losses from using a uniform kernel are small both in actual applications and in Monte Carlo simulations. Results from using triangular kernels are similar and available upon request.

[31]Specifically, Imbens and Kalyanaraman (2012) proposed an algorithm for computing the mean squared error (MSE) optimal RD bandwidth, while Card et al. (2015b) proposed its analog for the fuzzy RK setting, using asymptotic theory from Calonico et al. (2014).

[32]Both IK and CCT procedures involve a regularization term, which reflects the variance in the bias estimation and guards against the selection of large bandwidths.

threshold E_q^0. The variable D_i is an indicator that is set equal to 1 when earnings are above E_q^0 and 0 otherwise: $D_i = \mathbf{1}_{[\mathbf{E_i - E_q^0 > 0}]}$. As noted above, we control for normalized base period earnings relative to the threshold $(E_i - E_q^0)$ using local linear or quadratic polynomials (i.e., $\overline{p}$ is either equal to 1 or 2). To account for any effects of the business cycle and the Great Recession, we control for year×quarter fixed effects, ω_q, in all of our models. We also control for fixed effects for every week of each quarter (1 through 13), α_w, to account for the fact that subsequent labor market participation in post-leave quarters may differ depending on when during a particular quarter a leave claim is initiated (recall that we have exact claim effective dates, but observe employment and earnings at a quarterly level). The estimated change in the slope in the denominator of the ratio in equation (3.2) is given by θ_1. We show results with and without a vector of individual controls, X_i, which includes indicators for employee age categories (20-24, 25-29, 30-34, 35-39, 40-44), pre-claim employer industry (NAICS industry groups), and firm size (1-49, 50-99, 100-499, 500+). e_{iqw} is the unobserved error term, and we use heteroskedasticity robust standard errors, following Card et al. (2015a).

The second stage regression is:

$$Y_{iqw} = \pi_0 + \pi_1 \widehat{ln(b_{iq})} + \sum_{p=1}^{\overline{p}} \lambda_p (E_i - E_q^0)^p + \delta_q + \eta_w + \zeta' X_i + \varepsilon_{iqw} \quad \text{if} \quad |E_i - E_q^0| \leq h \quad (3.4)$$

for each woman i with a first bonding claim in year×quarter q in week of quarter w. Here, Y_{iqw} is an outcome, and $\widehat{ln(b_{iq})}$ is instrumented with the interaction between D_i and the polynomial in normalized base period earnings. The remainder of the variables are as defined before. The coefficient of interest, π_1, measures the effect of a 100 percent increase in the WBA on the outcome, and provides an estimate of γ_{RK} defined above.

Identifying assumptions. The identifying assumptions for inference using the RK design are: (1) in the vicinity of the earnings threshold, there is no change in the slope of the underlying direct relationship between base period earnings and the outcome of interest, and (2) the conditional density of base period earnings is continuously differentiable at the earnings threshold. These assumptions imply that individuals cannot perfectly sort at the earnings threshold (i.e., they cannot manipulate their earnings to end up on one or the other side of the threshold).

Importantly, since we only use data on women who make a bonding claim, differential selection into program take-up across the threshold would violate our identifying assumptions.[33] Lack of data on individuals who are eligible for a social insurance program but do not take it up is a common feature of RK studies (e.g., Landais, 2015, Card et al., 2015a, and Card et al., 2015b only use data on UI claimants, while Gelber et al., 2016 and Hansen et al., 2017 use data on SSDI and Workers' Compensation program claimants, respectively). Following the literature, we conduct standard tests of the identifying assumptions to address concerns about differential selection into take-up.

First, we show the frequency distribution of normalized base period earnings around the earnings threshold in Figure 3.3a. This graph uses \$100 bins and a \$5,000 bandwidth.[34] The histogram looks reasonably smooth, and we also perform formal tests to support this assertion. Specifically, we conduct a McCrary test (McCrary, 2008) for a discontinuity in the assignment variable at the kink, reporting the change in height at the kink and the standard error. We also test for a discontinuity in the first derivative of the p.d.f.

[33] While our quarterly earnings data include many individuals who are not PFL claimants, these data contain no demographic information, preventing us from identifying sub-groups who are plausibly eligible for PFL (i.e., mothers of infants or even women of childbearing age). Our calculations based on aggregate births data and employment estimates from the American Communities Survey (ACS) suggest that between 40 and 47 percent of all employed new mothers used CA-PFL bonding leave during 2005-2014 (Bana et al., 2018b). See also Pihl and Basso (2016) for similar estimates on program take-up.

[34] The results presented in Figure 3.3a are similar under alternative bandwidths.

of the assignment variable, following Card et al. (2012), Landais (2015), and Card et al. (2015b): we regress the number of observations in each bin on a 3rd order polynomial in normalized base period earnings, interacted with D, the indicator for being above the threshold. The coefficient on the interaction between D and the linear term, which tests for a change in the slope of the p.d.f., is reported in each panel, along with the standard error.

We do not detect any statistically significant discontinuities in either the frequency distribution or the slope change at the threshold.[35] Additionally, we have conducted separate McCrary tests for each distinct kink over our analysis time frame, and found that out of 16 possible coefficients, only two are statistically significant (for the last two kinks in the data). As we show below, our results are similar if we limit our analysis to claimants in 2005-2010, where we do not observe any significant discontinuities or slope changes at kink points. Thus, we do not think that differential sorting over time presents concerns for interpreting our main estimates.

Second, we check for any kinks in pre-determined covariates around the threshold. In Appendix Figure A.1, we use \$100 bins of normalized base period earnings and plot the mean employee age and firm size as well as the number of women in the health industry (the top industry in our data) in each bin. Results from regressions testing for a change in the slope of the relationship between the covariate and the running variable yield insignificant coefficients for employee age and firm size. The coefficient for the number of women in the health industry is statistically significant, but very small in magnitude.[36]

These figures provide support for the validity of the RK research design: We do not

[35] We follow Card et al. (2015b) to choose the order of the polynomial. We fit a series of polynomial models of different orders that allow for a discontinuity at the threshold and also allow the first and higher-order derivatives to vary at the threshold, and then select the model with the smallest Akaike Information Criterion (AIC) value (3rd order in our case).

[36] Specifically, the kink coefficients and standard errors are as follows: mean age -0.00002 (SE= 0.00002); mean firm size 0.04667 (SE= 0.0581); number in health industry -0.0073 (SE= 0.0029).

observe any evidence of sorting or underlying non-linearities around the kink point, which also argues against any differential selection into CA-PFL take-up across the earnings threshold.

3.5 Results

Main results. Figure 3.3b plots the empirical relationship between the natural log of the authorized WBA and normalized quarterly base period earnings. The empirical distribution of benefits is very similar to the benefit schedules depicted in Figure 3.1, with clear evidence of a kink at the threshold at which the maximum benefit begins. The first stage $F-$statistic is 2634.5.

Figure 3.3 shows graphs using our main outcome variables on the $y-$axes; we use $100 bins in the assignment variable and plot the mean outcome values in each bin. In Figure 3.4 and 3.5, we also graphically present the 2SLS estimates of π_1 and the 95% confidence intervals from equation (3.4), using specifications that implement different optimal bandwidth selection algorithms and controlling for first or second order polynomials in the running variable. We show results from models without and with individual controls (all models control for year$\times$quarter and week of quarter fixed effects). The specifications are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. Appendix Tables A.2 through A.6 present the corresponding point estimates and standard errors in table format, along with the first stage coefficients and standard errors (multiplied by 10^5 to reduce the number of leading zeros reported), the bandwidths, and the dependent variable means.[37] While the estimates just discussed report results from specifications that use the natural log of the benefit amount (as written in equation (3.4)), we show estimates from models that use the benefit amount in levels in Appendix Figure A.2 and A.3.[38] Lastly, Figure 3.6 plots the coefficients and 95% confidence intervals from local linear specifications that use all possible bandwidths in $500 increments of normalized quarterly base period earnings from $2,500 to $10,000.

Across the multiple RK specifications we consider, we find no evidence that a higher WBA increases maternity leave duration among new mothers. The upper bounds on the 95% confidence intervals of the estimates in Appendix Table A.2 allow us to rule out that a 10 percent increase in the WBA would increase leave duration by more than than 0.3 to 2.1 percent (or, elasticities from 0.03 to 0.21). Importantly, this finding is *not* explained by a highly skewed distribution of leave duration in which most women are "maxing out" their leave. In Figure 3.7, we plot the distribution of leave duration for women near the kink point ($5,000 bandwidth sample). We show the distribution of SDI leave, PFL leave, and combined SDI+PFL leave. About 16 percent of women take zero weeks of SDI leave (sub-figure a), which likely explains the mass at 6 weeks in the distribution of combined leave (sub-figure c). Conditional on taking PFL, about 80 percent of women use the entire 6 weeks (sub-figure b). But most women use both SDI and PFL to take less than the maximum amount of leave allowed on the two programs (16 weeks for women with uncomplicated vaginal deliveries).[39]

It also does not appear that leave benefits have any adverse consequences for subsequent maternal labor market outcomes. The estimates for the likelihood of employment in

[37] We report the main and pilot bandwidth, as in Card et al. (2015b). The pilot bandwidth is used in the bias estimation part of the bandwidth selection procedure. See Card et al. (2015b) for more details.

[38] Note that the sample sizes differ across the outcomes we consider because we use different sets of years for estimation; see Section 3.3.

[39] There is no statistically significant kink in the relationship between the share of women taking SDI and base period earnings (results available upon request).

quarter 2 after the claim and on the change in log earnings are insignificant in nearly all of the specifications (Appendix Tables A.3 and A.5). When we consider employment in the pre-leave firm *conditional* on any employment in quarter 2 post-claim, however, we find robust and consistently positive treatment coefficients, which are significant at the 1% level in 8 out of the 12 models (Appendix Table A.4). The range of estimates suggests that a 10 percent increase in the WBA raises the likelihood of return to the pre-leave firm by 0.3 to 4.2 percentage points (0.3 to 5 percent at the sample mean).[40]

On the whole, the evidence on post-leave labor market outcomes is inconsistent with an income effect channel (which would reduce maternal labor supply; see Wingender and LaLumia, 2017). Instead, these results suggest that higher pay during leave might improve employee morale and possibly promotes firm loyalty, such that a mother is more likely to return to her pre-leave firm rather than search for a new employer.

Further, when we examine subsequent bonding claims, we find a robust positive effect. Our estimates in Appendix Table A.6 indicate that a 10 percent increase in the WBA raises the likelihood of a future bonding claim by 0.8 to 1.6 percentage points (3 to 7 percent at the sample mean). This effect, combined with evidence on the increased likelihood of return to the pre-leave firm, echoes conclusions in Bana et al. (2018d), who document that firm-specific factors drive a large share of the variation in PFL use. Our results suggest that a higher benefit amount leads mothers to return to the employers at which they make their first bonding claims instead of switching to other firms which may have lower leave-taking rates.

It is also possible that the increase in repeat claiming could operate through an effect on subsequent fertility, which we do not observe in our data. However, past research from other countries offers mixed evidence on the relationship between PFL and maternal

[40]We have also examined unconditional employment in the pre-leave firm, finding no significant impacts (results available upon request).

fertility (Dahl et al., 2016; Lalive and Zweimüller, 2009), so we do not believe this to be the primary channel. A third possibility is that even in the absence of changes to employment or fertility, mothers with a higher benefit have a better experience during leave and are more likely to use the program again than those with lower payments.

Timing of effects. In Appendix Figure A.4, we examine how the impact of the WBA evolves over the quarters following the claim. The graphs show the coefficients and 95% confidence intervals from separate regression models that use the fuzzy IK bandwidth with a local linear polynomial specifications. In sub-figures (a) and (b) we consider as outcomes indicators for employment and employment in the pre-leave firm (conditional on any employment) in quarters 2 through 5 after the claim, respectively. In sub-figure (c), we use an indicator for any subsequent bonding claim *by* the quarter listed on the x-axis (4 through 20).

We find no significant effects on the likelihood of any employment in quarter 2, 4, or 5 after the claim. The effect on employment in quarter 3 post-claim is statistically significant, but we note that this is largely due to the wide bandwidth chosen by the fuzzy IK algorithm (the effect is not significant in any of the other specifications). When we consider the effect on employment in the pre-leave firm conditional on any employment, we find that it is large and statistically significant in both quarters 2 and 3 post-claim, becoming insignificant in the subsequent quarters. The impact on subsequent bonding materializes in quarter 8 after the claim, which is consistent with mothers returning to their pre-leave employers in quarter 2, working for the next four quarters to set the base period earnings for their next claim, and then making a subsequent claim 3 quarters later, which is the approximate duration of a pregnancy.

Heterogeneity and subsample analysis. We have analyzed heterogeneity in the effects of benefits across employee and employer characteristics (age, firm size, and industry groups), finding no consistent patterns. The lack of significant heterogeneity across women in firms that have 50 or more employees and their counterparts in smaller firms is notable in light of the fact that workers in the former group are more likely to be eligible for job protection through the FMLA or the CFRA. Our results suggest that eligibility for government-mandated job protection does not contribute to differences in the impacts of PFL benefits, at least in our high-earning RK sample.

Additionally, as discussed in Section 3.2, one might be concerned that some employers are undoing the CA-PFL benefit cap—and thereby weakening our RK design—by supplementing PFL benefits so that employees on leave receive 100 percent of their salary (or at least more than 55 percent of their salary). Unfortunately, our data do not report such payments, nor could we locate any external evidence that such practices are common. Instead, to assess whether this issue may be impacting our main results, we examine subsamples where it is least likely to be important. First, employees who made claims soon after the implementation of CA-PFL (in 2005-2010) are less likely to have received such payments as it takes time for new programs to be incorporated in firm benefit plans, and media coverage of existing employer-provided paid leave policies (mostly at tech companies in California) suggests that such policies were rare prior to 2010.[41] Second, workers in smaller firms are less likely to have access to such generous supplemental funds, as these employers tend to have more modest human resource infrastructures. We therefore replicate Figure 3.6 for the following subsamples: claimants in 2005-2010, claimants in non-tech companies (we drop NAICS industry code 51, Information), and claimants in firms with less than 1,000 workers. The results are reported in Appendix Figures A.5, A.6, and A.7, respectively. In all cases, the pattern of findings for these

[41] See, for example: `https://tcf.org/content/report/tech-companies-paid-leave/`.

subsamples are similar to those for the entire sample, although the estimates are less precise. Put differently, we find no suggestion that supplemental payments that remove the kink are driving the main results.

Permutation tests. An important concern for the RK design is the possibility of spurious effects resulting from non-linearities in the underlying relationship between the outcome and the assignment variable. To address this concern, we perform a series of permutation tests, as proposed in recent work by Ganong and Jäger (2017). The idea is to estimate RK models using placebo kinks at various points in the distribution of base period earnings. Specifically, we use a sample of women making their first bonding claims with base period earnings within a \$40,000 window of the true kink point, and estimate 150 RK models for each outcome, using a \$4,000 bandwidth surrounding each placebo kink point. All regressions include year×quarter and week-of-quarter of the claim fixed effects, as in the main specifications without individual-level controls.[42] Note that the permutation tests are estimated as reduced form models. As such, the placebo kink coefficients are of the opposite sign from those in our main IV models (which are scaled by negative first stage coefficients).

Figure 3.8 presents the results, where the placebo kink points are denoted on the $x-$axis normalized relative to the true kink point (i.e., the true kink point is at 0). For log leave duration and change in log earnings, we do not find any statistically significant estimates using any of the placebo kinks that we consider. For employment in quarter 2 post-leave, we do observe significant coefficients when we use placebo kinks \$2,000 to \$4,000 less than the true kink, suggesting that there may be non-linearities in this outcome function that may bias the results. By contrast, when we consider the outcomes for which we find the most robust effects—indicators for employment in the same firm

[42]We have also estimated the permutation tests with individual-level controls, which yield similar results and are available upon request.

conditional on any employment and for a subsequent bonding claim—we do not observe any significant placebo coefficients, while the coefficients in close vicinity to the true kink point are consistently statistically significant, as in our main results.

Difference-in-difference models. As an alternative to the RK design, we examine estimates from difference-in-difference models, which leverage non-linear variation over time in benefit amounts. Specifically, we use our baseline analysis sample of women with base period quarterly earnings within a $\$10,000$ bandwidth of the kink point in every year, and split them into groups defined by \$1,000 bins of real (\$2014) base period earnings. We then estimate versions of the following model:

$$Y_{iqw} = \varsigma_0 + \varsigma_1 ln(b_{iq}) + \varrho_q + \varphi_{E_{iq}} \times q + \vartheta_w + \upsilon_{iqw} \tag{3.5}$$

for each woman i with a first bonding claim in year×quarter q in week of quarter w. $\varphi_{E_{iq}}$ are fixed effects for the \$1,000 base period earnings bins, which in some specifications we interact with linear trends in q. As before, we include year×quarter and week-of-quarter fixed effects. The coefficient ς_1 represents the effect of a 100 percent increase in the WBA on the outcome of interest, and is identified using variation in benefit amounts *within* \$1,000 bins of women's base period quarterly earnings.

Appendix Table A.7 presents the results from these models, for each of our five main outcomes.[43] Broadly speaking, these results—which are based on a different identification strategy that uses a sample of women with a wider range of base period earnings than our primary RK specifications—are consistent with our main findings. The coefficient for the effect of the WBA on leave duration is now statistically significant, but the magnitude is small and comparable to the RK estimates: a 10 percent increase in the WBA increases

[43] We have also estimated analogous difference-in-difference models, using the WBA in levels rather than in logs. Results are similar and available upon request.

maternity leave duration by only 0.2 percent. We also find that a 10 percent rise in the WBA is associated with a 0.5 percentage point decline in the likelihood of employment in quarter 2 post-claim, which is very small relative to the 87 percent mean (see column (4) of Table 3.1). Consistent with the RK results, we further show that the WBA is *positively* associated with the likelihood of return to the pre-leave employer conditional on any employment, with a 10 percent increase in the WBA leading to a 2 percentage point rise in this outcome (which is in the range of estimates suggested by the RK models). We also now find that a 10 percent rise in the WBA results in a significant 1.5 percent increase in the earnings change from before to after the leave, an estimate that is larger than those suggested by the RK specifications. Lastly, we see that a 10 percent higher WBA leads to a 0.8 percentage point higher likelihood of having a subsequent bonding claim; this estimate is comparable to those from the RK models. In sum, our results are robust to using an alternative empirical strategy to the RK method.

3.6 Conclusion

According to the most recent statistics, only 14 percent of American workers have access to paid family leave through their employers.[44] The fact that the U.S. does not provide any paid maternity or family leave at the national level—and, in doing so, is an outlier when compared to other developed countries—has received substantial attention from politicians, policy advocates, and the press. There exists, however, some access to government-provided unpaid family leave through the FMLA, implying that understanding the specific consequences of *monetary benefits* during leave is of first-order importance to both researchers and policy-makers. In this paper, we attempt to make progress on this question by estimating the causal effects of PFL wage replacement

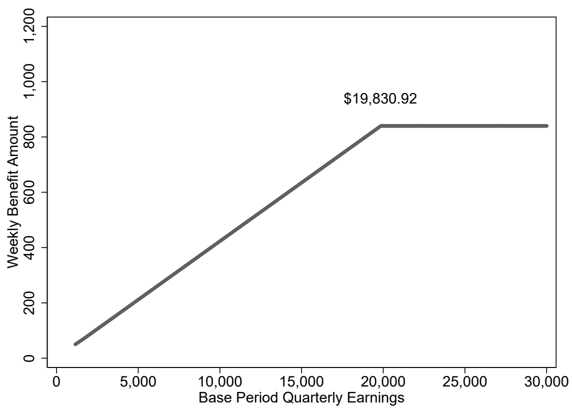
[44]See: `http://www.nationalpartnership.org/issues/work-family/paid-leave.html`.

rates on maternal leave duration, labor market outcomes, and future leave-taking among high-earning mothers in California, the first state to implement its own PFL program.

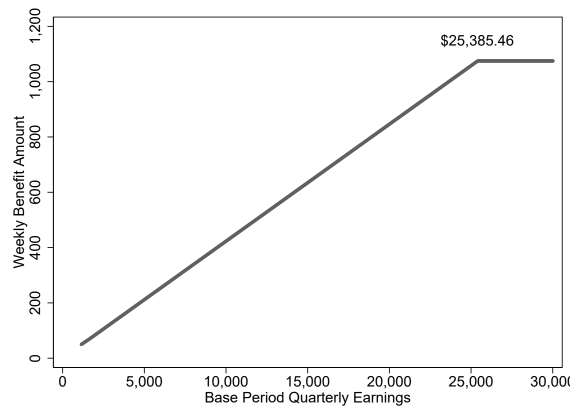
We leverage detailed administrative data on the universe of PFL claims linked to quarterly earnings records together with an RK research design. Comparing outcomes of mothers with base period earnings below and above the maximum benefit threshold, we find that higher benefits have zero impacts on leave duration, a result that contrasts sharply with prior evidence from other social insurance programs. We also find some evidence of positive impacts on the likelihood that mothers return to their pre-leave employers instead of switching to new firms: conditional on any employment in quarter 2 post-claim, a 10 percent increase in the WBA raises the likelihood of employment at the pre-leave employer by 0.3 to 5 percent, depending on specification. Further, benefits during the first period of paid family leave predict future program use. An additional 10 percent in benefits is associated with a 3 to 7 percent increase in the probability of having a subsequent PFL claim in the following three years.

The results reported in this paper serve as an important step toward understanding the influence of benefit levels on leave duration, subsequent labor market outcomes, and future leave-taking for high-earning women in the United States, who are disproportionately affected by the "motherhood wage penalty" (Anderson et al., 2002; Bertrand et al., 2010; Hotchkiss et al., 2017; Chung et al., 2017). Our results assuage concerns that wage replacement during family leave may have unintended negative consequences for mothers' future labor market outcomes through an increase in time away from work, at least among these women. Of course, it is important to recognize that these findings may be specific to the relatively short statutory leave duration permitted under CA-PFL; benefits provided in the context of much longer leaves—such as those in many European countries—may have different effects. Our RK estimates also generate insights on the implications of benefit changes around the maximum benefit threshold. This evidence is valuable because all existing state PFL programs, as well as the national FAMILY Act proposal, feature similar kinked benefit schedules. As other jurisdictions have opted for different replacement rates and benefit caps than California, future research on these other policies will further contribute to our understanding about the relationships between PFL benefits and outcomes across the earnings distribution.

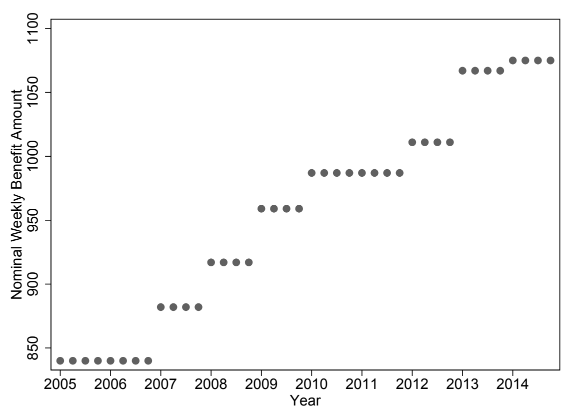
Figure 3.1: PFL/SDI Benefit Schedule in 2005 and 2014 and the Maximum Weekly Benefit Amount Over Time



(a) 2005 Schedule



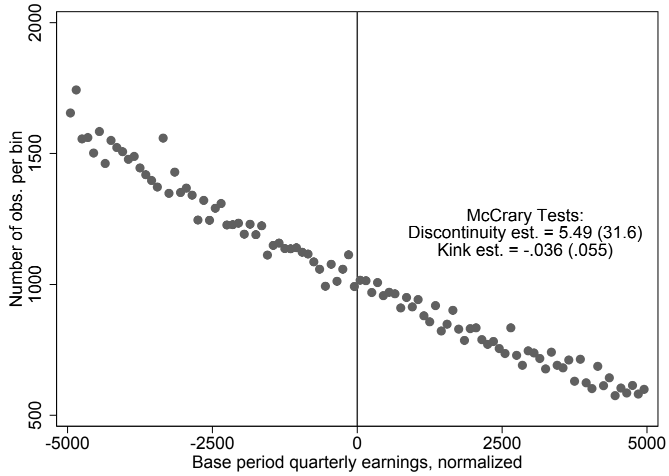
(b) 2014 Schedule



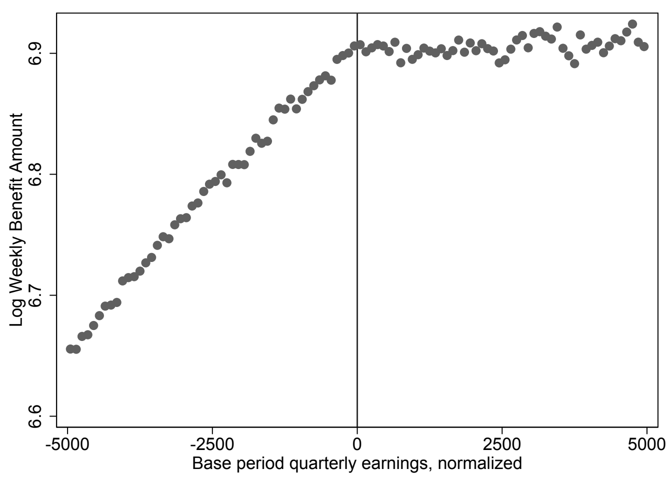
(c) Maximum WBA by Quarter

Notes: Sub-figures (a) and (b) plot nominal quarterly base period earnings on the $x-$axis and the nominal weekly benefit amount on the $y-$axis for 2005 and 2014, respectively, with the earnings threshold at which the maximum benefit begins labeled in each sub-figure. Sub-figure (c) plots the maximum weekly benefit amount by quarter in nominal dollars over the time period 2005 quarter 1 through 2014 quarter 4.

Figure 3.2: Frequency Distribution of Base Period Earnings Around the Earnings Threshold and First Stage



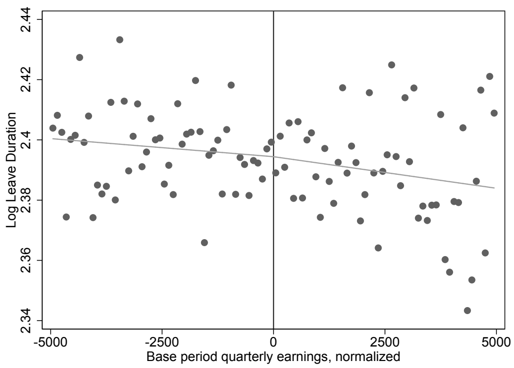
(a) Frequency Distribution



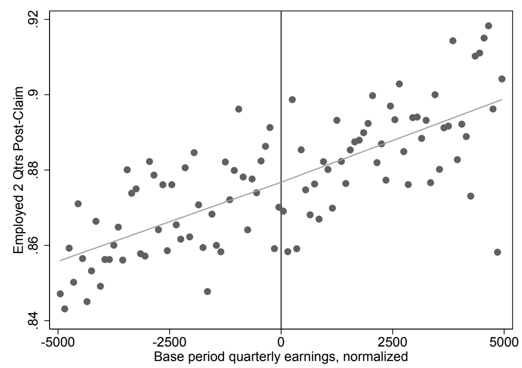
(b) First Stage

Notes: Sub-figure (a) shows the frequency distribution for women. The $x-$axis plots normalized base period quarterly earnings (relative to the earnings threshold in each year) in bins, using \$100 bins, and with a \$5,000 bandwidth. We display two tests of the identifying assumptions of the RK design. The first is a standard McCrary test of the discontinuity of the p.d.f. of the assignment variable ("Discontinuity est."). The second is a test for discontinuity in the first derivative of the p.d.f. ("Kink est."). For both, we report the estimate and the standard error in parentheses. We follow Card et al. (2015b) to choose the order of the polynomial in these tests. We fit a series of polynomial models of different orders that impose continuity but allow the first and higher-order derivatives to vary at the threshold, and then select the model with the smallest Akaike Information Criterion (AIC) value (3rd order in our case). Sub-figure (b) shows the empirical relationship between the log weekly benefit amount received and normalized base period earnings for women. The $x-$axis plots normalized base period quarterly earnings (in terms of distance to the earnings threshold) in bins, using \$100 bins.

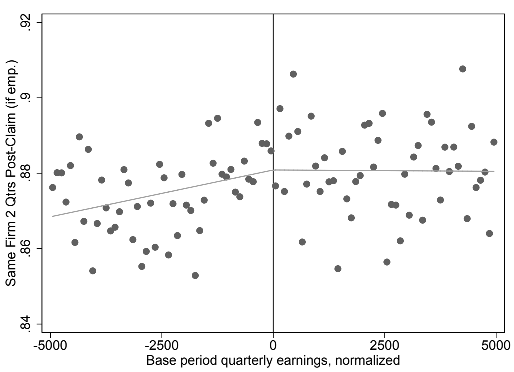
Figure 3.3: RK Figures for Main Outcomes



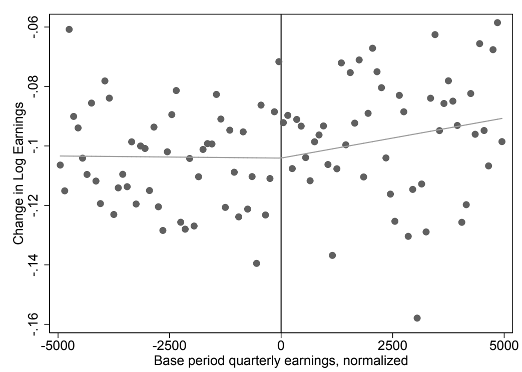
(a) Log Leave Duration



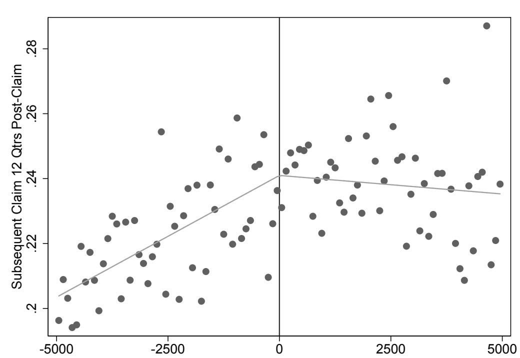
(b) Employed, 2 Qtrs. Post-Claim



(c) Same Firm (if Employed)



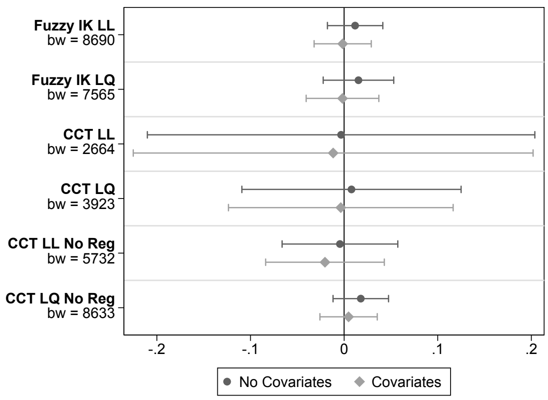
(d) Δ Log Earnings



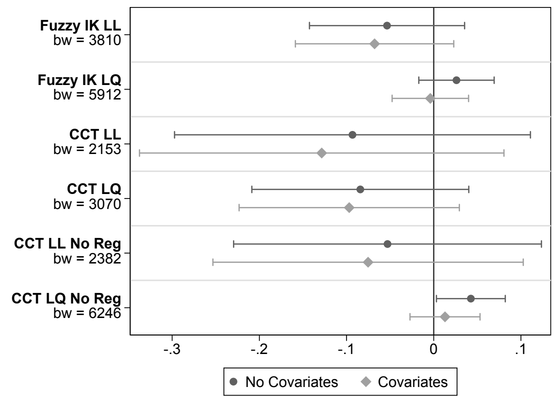
(e) Any Subsequent Bonding Claim

Notes: The $x-$axis plots normalized base period quarterly earnings (relative to the earnings threshold in each year) in bins, using \$100 bins. The $y-$axis plots the mean of the outcome in each bin. The outcomes are: (1) natural log of leave duration in weeks, (2) an indicator for the woman being employed in quarter 2 after the claim, (3) an indicator for the woman being employed in her pre-claim firm in quarter 2 after the claim, conditional on any employment in that quarter, (4) the change in log earnings from quarters 2-5 before the claim to quarters 2-5 after the claim, and (5) an indicator for any subsequent bonding claim in the 12 quarters following the first claim.

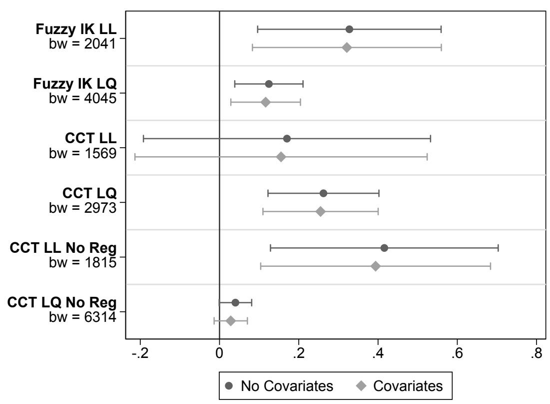
Figure 3.4: RK Estimates for Main Outcomes Using Different Specifications



(a) Log Leave Duration



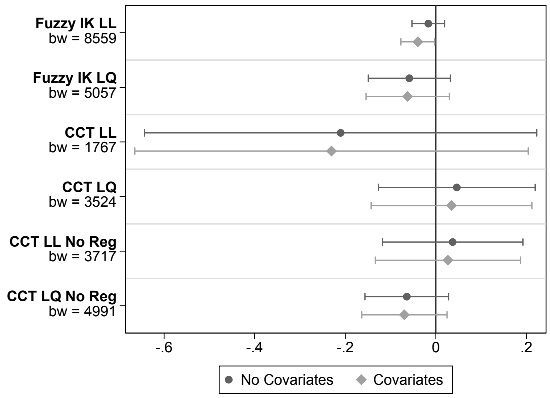
(b) Employed, 2 Qtrs. Post-Claim



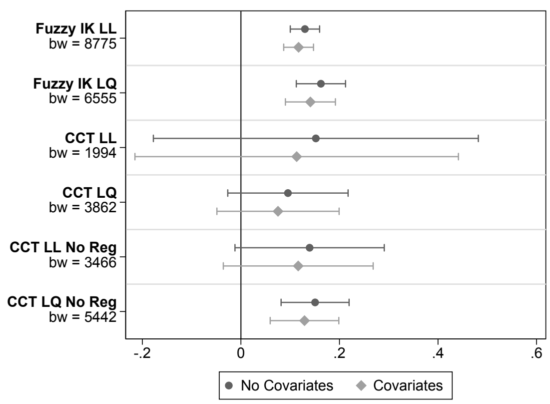
(c) Same Firm (if Employed)

Notes: These figures show the coefficients and 95% confidence intervals (as horizontal bars) from different RK specifications, estimated separately with and without individual-level controls. The coefficients and standard errors from these regressions are reported in Appendix Tables A.2, A.3, A.4, A.5, and A.6. See notes under Figure 3.3 for more details about the outcomes. All regressions include year×quarter and week-of-quarter of the claim fixed effects. The specifications with individual controls include the following variables: indicators for employee age categories (20-24, 25-29, 30-34, 35-39, 40-44), dummies for pre-claim employer industry (NAICS industry groups), and dummies for employer size (1-49, 50-99, 100-499, 500+). The specification models are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. The optimal bandwidths from each specification are listed.

Figure 3.5: RK Estimates for Main Outcomes Using Different Specifications



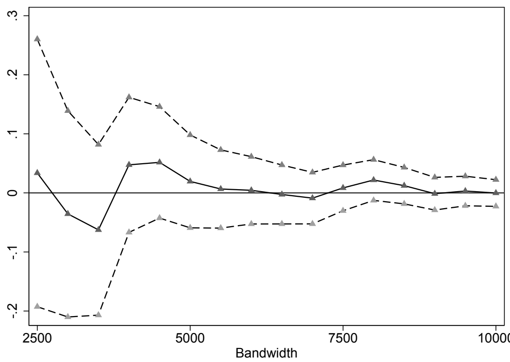
(a) Δ Log Earnings



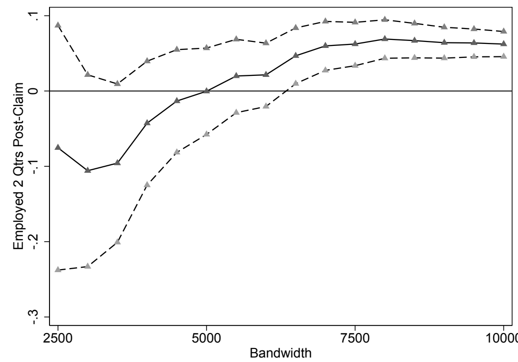
(b) Any Subsequent Bonding Claim

Notes: These figures show the coefficients and 95% confidence intervals (as horizontal bars) from different RK specifications, estimated separately with and without individual-level controls. The coefficients and standard errors from these regressions are reported in Appendix Tables A.2, A.3, A.4, A.5, and A.6. See notes under Figure 3.3 for more details about the outcomes. All regressions include year×quarter and week-of-quarter of the claim fixed effects. The specifications with individual controls include the following variables: indicators for employee age categories (20-24, 25-29, 30-34, 35-39, 40-44), dummies for pre-claim employer industry (NAICS industry groups), and dummies for employer size (1-49, 50-99, 100-499, 500+). The specification models are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. The optimal bandwidths from each specification are listed.

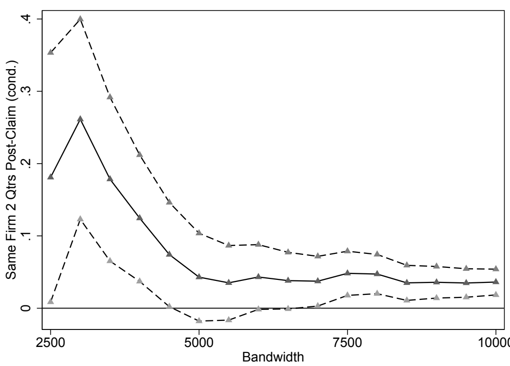
Figure 3.6: RK Estimates for Main Outcomes Using Different Bandwidths



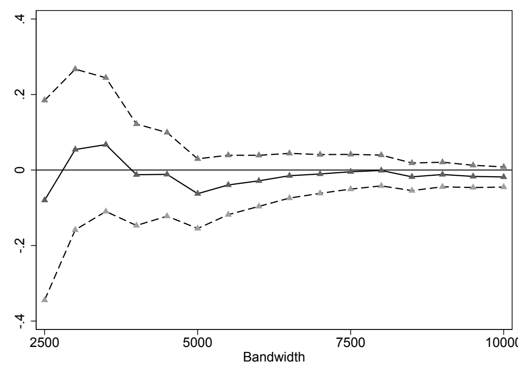
(a) Log Leave Duration



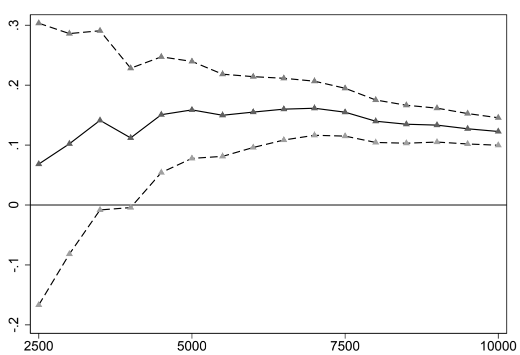
(b) Employed, 2 Qtrs. Post-Claim



(c) Same Firm (if Employed)



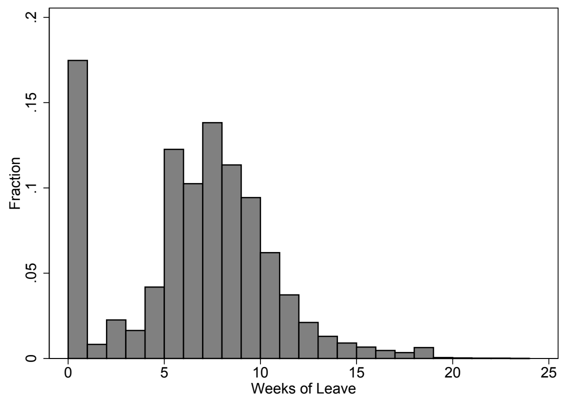
(d) Δ Log Earnings



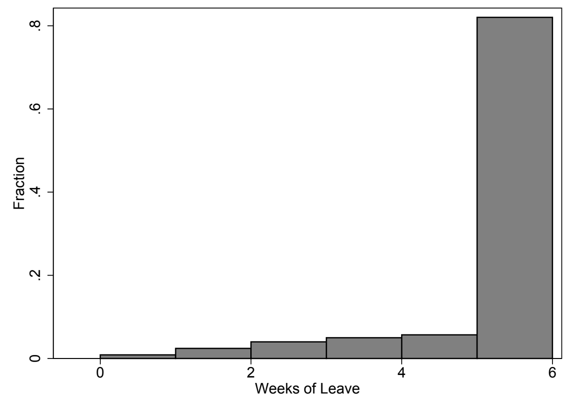
(e) Any Subsequent Bonding Claim

Notes: These figures show the coefficients (as dark gray triangles) and 95 percent confidence intervals (as light gray triangles) from RK specifications that use different bandwidths in increments of \$500 of normalized quarterly base period earnings (denoted on the x−axis) and local linear polynomials. All regressions include year×quarter and week-of-quarter of the claim fixed effects. See notes under Figure 3.3 for more details about the outcomes.

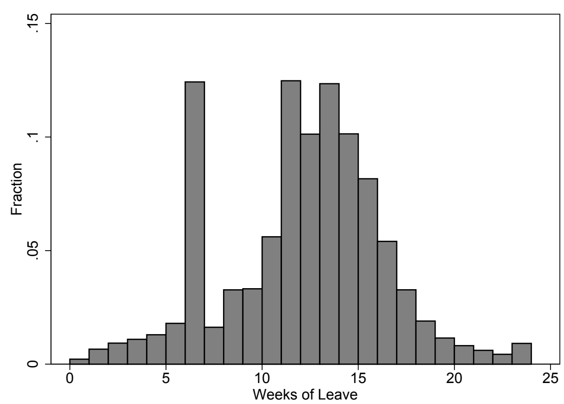
Figure 3.7: Distribution of Leave Duration for Women with Earnings Near the Threshold



(a) SDI Only



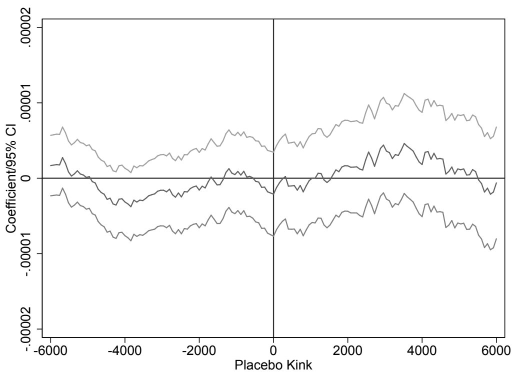
(b) PFL Only



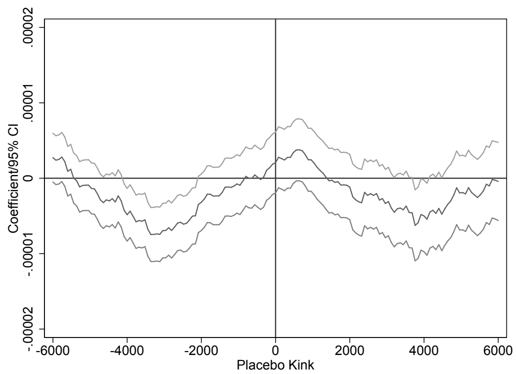
(c) Total (SDI+PFL)

Notes: These figures plot the distributions of leave duration for women with pre-claim earnings within a $5,000 bandwidth surrounding the kink point.

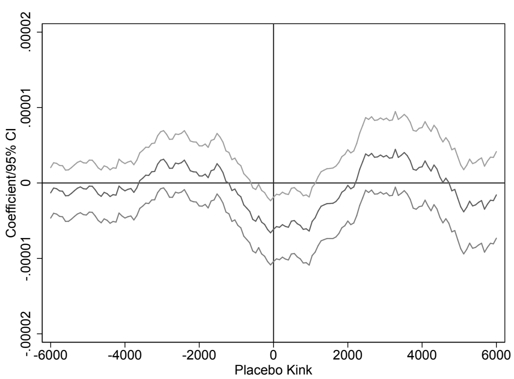
Figure 3.8: Permutation Tests



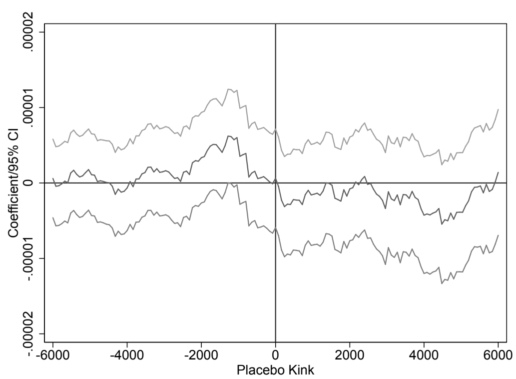
(a) Log Leave Duration



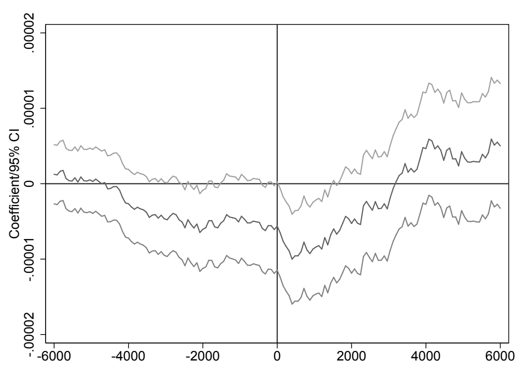
(b) Employed, 2 Qtrs. Post-Claim



(c) Same Firm (if Employed)



(d) Δ Log Earnings



(e) Any Subsequent Bonding Claim

Notes: These figures show the coefficients (as dark gray lines) and 95 percent confidence intervals (as light gray lines) from placebo RK specifications with a placebo kink specified in terms of distance from the true kink point (i.e., the true kink point is at 0 on the $x-$axis). To estimate the placebo RK specifications, we use a sample of women making their first bonding claims with base period earnings within a \$40,000 window of the true kink point, and estimate placebo RK models for each outcome, using a \$4,000 bandwidth surrounding each placebo kink point. All regressions include year×quarter and week-of-quarter of the claim fixed effects, as in the main specifications without individual-level controls.

Table 3.1: Descriptive Statistics

	2500	5000	7500	10000
Age	32.80	32.69	32.53	32.20
	(4.10)	(4.12)	(4.20)	(4.34)
Firm Size 1-49	0.19	0.19	0.20	0.21
	(0.39)	(0.39)	(0.40)	(0.41)
Firm Size 50-99	0.08	0.08	0.08	0.08
	(0.26)	(0.27)	(0.27)	(0.27)
Firm Size 100-499	0.20	0.21	0.21	0.21
	(0.40)	(0.40)	(0.41)	(0.41)
Firm Size 500+	0.53	0.52	0.51	0.50
	(0.50)	(0.50)	(0.50)	(0.50)
Weekly Benefit Amount ($2014)	975.29	932.99	878.18	807.50
	(110.50)	(127.10)	(154.74)	(188.66)
Base Period Earnings ($2014)	24158.72	23460.08	22311.82	20624.44
	(1774.89)	(3217.20)	(4615.00)	(5905.67)
Health Industry	0.33	0.32	0.30	0.28
	(0.47)	(0.47)	(0.46)	(0.45)
Total Leave Duration	11.94	11.95	11.95	11.97
	(4.22)	(4.23)	(4.22)	(4.23)
Employed 2 Qtrs. Post-Claim	0.88	0.87	0.87	0.86
	(0.33)	(0.33)	(0.34)	(0.35)
Same Firm 2 Qtrs. Post-Claim (cond.)	0.88	0.88	0.87	0.87
	(0.33)	(0.33)	(0.33)	(0.34)
Employed 3 Qtrs. Post-Claim	0.86	0.86	0.85	0.84
	(0.35)	(0.35)	(0.36)	(0.37)
Same Firm 3 Qtrs. Post-Claim (cond.)	0.84	0.83	0.83	0.83
	(0.37)	(0.37)	(0.37)	(0.38)
Employed 4 Qtrs. Post-Claim	0.85	0.85	0.84	0.83
	(0.36)	(0.36)	(0.37)	(0.38)
Same Firm 4 Qtrs. Post-Claim (cond.)	0.80	0.80	0.79	0.79
	(0.40)	(0.40)	(0.40)	(0.41)
Change in Log Earnings	-0.10	-0.10	-0.10	-0.10
	(0.46)	(0.48)	(0.48)	(0.49)
Subsequent Claim 12 Qtrs. Post-Claim	0.23	0.23	0.22	0.20
	(0.42)	(0.42)	(0.41)	(0.40)
Observations	50,802	104,016	164,163	240,541

Notes: This table presents the means and standard deviations (in parentheses) of some of the key variables for women making their first PFL bonding claims during 2005-2014 with base period earnings within the bandwidths listed at the top of each column. We make the following sample restrictions: (1) We only include women who are aged 20-44 at the time of the first bonding claim; (2) We drop women employed in industries in which employees are least likely to be subject to the SDI tax—private household workers, elementary and secondary school teachers, and public administration; (3) We drop women with zero total earnings in the base period quarters.

Chapter 4

Unequal Use of Social Insurance: The Role of Employers

4.1 Introduction

The dramatic rise in U.S. inequality in recent decades has motivated a burgeoning literature on its causes and consequences along a number of dimensions, including wages (Acemoglu and Autor, 2011), income (Chetty et al., 2014), wealth (Saez and Zucman, 2016), health (Currie and Walker, 2011; Chetty et al., 2016), and family structure (Lundberg, 2015). When it comes to the growth in earnings inequality, recent research emphasizes the role of *employers*, finding that most of the increase is due to widening earnings dispersion between, rather than within, firms (Song et al., 2018). But less is known about the influence of employers on other aspects of inequality among Americans, or about *non-wage* differences between high-paying and low-paying firms. In this paper, we aim to understand how firms contribute to inequality in the use of public short-term leave-taking social insurance programs, which allow individuals to take partially paid leave for their own medical issues or to care for new children or ill family members.

A growing body of evidence demonstrates that access to temporary social insurance has beneficial labor market and health effects on workers and their families (e.g., Rossin-Slater, 2018b; Olivetti and Petrongolo, 2017b; Stearns, 2015; ?), and can even generate positive externalities for the broader population (Stearns and White, 2018). However, the availability of short-term disability insurance (DI) and paid family leave (PFL) is highly limited in the United States. There is no federal legislation, and only five states have implemented public programs.[1] Most firms do not provide their own private benefits either, or if they do, they do not necessarily offer them to all of their employees. According to 2017 data, only about one third of all firms offer any paid maternity leave to workers, and only 17 percent offer paid paternity leave (Kurani et al., 2017). Overall, just 15 percent of workers have access to PFL and 39 percent have access to short-term DI.[2]

In addition to being limited, access to and the use of short-term social insurance in the U.S. is highly unequal. Only 6 and 19 percent of workers in the bottom quartile of the wage distribution have access to employer-provided PFL and short-term DI, respectively, compared to 25 and 54 percent of workers in the top quartile. Even in states with government programs, not all workers are equally able to take advantage of public benefits. For instance, despite the almost universal eligibility of workers in California, DI and PFL take-up rates are still substantially different across industries, firm sizes, and earnings quartiles for both men and women (Bana et al., 2018c). As most workers learn about public social insurance benefits through their employers, and polls document that lack of awareness about these programs is a major barrier to take-up (DiCamillo and Field, 2015), insights into the relationship between firm characteristics

[1] California, New York, New Jersey, and Rhode Island have both short-term DI and PFL programs. Hawaii has a short-term DI program but no PFL. Washington state, Washington, D.C., and Massachusetts have enacted paid family and medical leave legislation set to go into effect in the coming years.

[2] Source: Bureau of Labor Statistics, National Compensation Survey, March 2017, `https://www.bls.gov/ncs/ebs/benefits/2017/benefits_tab.htm`.

and program use are critical for understanding the drivers of these disparities.

This paper uses ten years of administrative data from California to provide the first evidence on the role of firms in explaining differences in short-term social insurance take-up. Drawing on a well-established literature that demonstrates that observably similar firms pay observably similar workers different wages (i.e., employer-specific wage premiums, or "firm fixed effects") (see, e.g.: Abowd et al., 1999; Card et al., 2013, 2016a; Barth et al., 2016; Card et al., 2018; Sorkin, 2018; Song et al., 2018), we analyze the relationship between the employer earnings premium and the share of employees within a firm who take DI or PFL in any given year. Whether firms with higher earnings premiums are more or less conducive to benefit take-up is theoretically ambiguous. Workers at higher premium firms might face a higher opportunity cost of taking leave, or be more likely to have access to private DI or PFL benefits that could crowd-out the use of public programs. But employers that offer private benefits may have a particularly strong incentive to encourage public benefit take-up, as it can lower the cost to the firm. Higher earnings premium firms—which are likely to be more innovative and productive than their lower-premium counterparts (Van Reenen, 1996; Faggio et al., 2010; Barth et al., 2016)—may also view their wage setting policies as complements to creating a workplace culture conducive to leave-taking.

To answer this question, we combine two data sets from the California Employment Development Department (CA EDD): the universe of DI and PFL claims over fiscal years 2004-2013, and quarterly earnings data for nearly all California employees from 2000 to 2014. Our empirical strategy involves two main steps. First, we estimate employer earnings premiums using the seminal Abowd, Kramarz, and Margolis (1999) (AKM) methodology that includes both worker and firm fixed effects to account for non-random sorting of workers across firms. Second, we aggregate the data to an employer level panel and estimate Poisson regressions of the number of social insurance claims within a firm in a given year on the firm earnings premium, controlling for firm size, industry and year fixed effects, and the percentage of female employees in each industry-year.

We find strong evidence that public temporary social insurance program take-up is higher in firms with relatively higher earnings premiums. A one standard deviation increase in the firm earnings premium is associated with a 57 percent increase in the incidence rate of claims. The effect of the firm premium is similar for claims made by men and women, and exists for both DI and PFL. We also show that the effect is largest for workers in the lower half of the employer-specific earnings distribution, suggesting that a firm's premium is particularly important in determining the non-wage benefit use of its lowest-earning employees. Although high-premium firms have higher claim rates relative to low-premium firms, they also have lower average leave durations and higher employee retention rates following periods of leave.

The results indicate that characteristics of firm culture that are reflected in the firm earnings premium may be key to increasing take-up rates of public social insurance in California. If all firms behaved as those in the top third of the firm premium distribution, a back-of-the-envelope calculation suggests that take-up rates for DI and PFL would increase by 25 and 29 percent, respectively.[3] By contrast, prior research demonstrates that specific policy levers—such as the wage replacement rate—have limited effects on take-up. Ziebarth (2013) shows that changes in wage replacement rates do not significantly affect take-up rates of a DI program that covers work absences longer than six weeks, while Ziebarth and Karlsson (2010) find that a large cut in the sick pay replacement rate in Germany had a relatively small impact on leave use, and only for a sub-group of workers with a limited history of work absences. In Japan, Asai (2015b)

[3]These calculations assume that claim rates are specific to three firms sizes (5-24, 25-99, and 100+ average employees), seventeen industries, and three terciles. The thought experiment reported here increases the claim rate in the first two terciles to the third tercile within specific firm size and industry categories. In other words, differential claim rates by firm size and industry are held constant.

finds that an increase in the maternity leave wage replacement rate has no effect on job continuity or leave duration among new mothers. Finally, in California, Bana et al. (2018a) show that a higher replacement rate does not increase PFL duration among high-earning mothers.

Our paper contributes to a growing literature on the determinants of public short-term leave take-up, which in the U.S. has mostly focused on the implementation of California's first-in-the-nation PFL program in 2004 (Rossin-Slater et al., 2013b; Das and Polachek, 2015b; Baum and Ruhm, 2016b; Bartel et al., 2018b).[4] Outside the U.S., many studies examine the effects of extensions in PFL policies (or, less frequently, introductions of new programs) on parental leave-taking and labor market outcomes (see Rossin-Slater, 2018b; Olivetti and Petrongolo, 2017b for recent overviews), but less is known about the use of temporary DI programs. In general, the existing studies find that very short-term sick leave use is positively correlated with the generosity of the benefits, while the relationship with longer periods of leave is less clear (Pettersson-Lidbom and Thoursie, 2013; Henrekson and Persson, 2004; Johansson and Palme, 2005; Dale-Olsen, 2014; Ziebarth and Karlsson, 2010; Ziebarth, 2013).

Moreover, we know little about other *non-policy-driven* determinants of temporary social insurance take-up.[5] Research on the importance of workplace culture in promoting work-family balance often relies on case studies and small samples, and cannot shed

[4]The small literature on state DI programs is largely focused on pregnacy-related coverage. Stearns (2015) exploits a law that required state DI programs to start covering pregnancy as a disability to look at the impact of benefits on infant health. Campbell et al. (2018) estimate the impact of pregnancy coverage under DI in Rhode Island on maternal labor supply and other outcomes. There is also a substantial literature on the effects of long-term disability (which covers permanent withdrawal from the labor market) on labor supply in the U.S. (e.g., Gruber, 2000; Autor and Duggan, 2003b; Chen and van der Klaauw, 2008).

[5]There is a small literature on the correlates with absenteeism, but these papers focus on very short-term absences (e.g., individual days) and are not necessarily relevant for studying PFL or DI. In these settings, absenteeism is often used as a proxy for effort. Dionne and Dostie (2007) find workplace conditions, including standard schedules, work at home options, and reduced workweeks are correlated with reduced absenteeism. Employment protection increases absenteeism as well (Riphahn, 2004; Ichino and Riphahn, 2005).

light on the characteristics of firms that support benefit take-up on a broader scale (Clark, 2001; Kelly et al., 2011; Moen et al., 2016). More relevant to our work, Dahl et al. (2014) find large peer effects in the take-up of publicly provided paternity leave in Norway, arguing that increased knowledge about employer reactions to leave is a primary mechanism. A separate literature on firm-specific premiums has quantified their importance in driving wage inequality (Card et al., 2013, 2016a; Song et al., 2018), but less is known about non-wage differences between high-premium and low-premium firms.[6] This paper bridges this gap by documenting a strong and robust association between employer earnings premiums and the use of temporary paid leave. Our findings suggest that firm-specific factors not only explain a substantial part of earnings dispersion, but also drive disparities in the use of public social insurance benefits.

4.2 Temporary Social Insurance in California

California's State Disability Insurance (SDI) is a partial wage-replacement insurance plan for workers in the state. Participation in the SDI program is mandatory for most private sector employees, and over 18 million workers are currently covered. The SDI program is funded entirely through employee payroll tax deductions and currently consists of two types of benefits: Disability Insurance (DI) and Paid Family Leave (PFL). Work requirements for coverage are quite low. Eligible individuals must have earned at least \$300 in taxable wages in a base period 5 to 18 months before the start of the claim, and eligibility is not employer-specific. The 2018 SDI tax rate is 1 percent on the first \$114,967 earned, and is not experience rated. During a claim, workers receive 55 percent

[6]Several recent papers examine the role of between- and within- firm factors on the gender wage gap. Hotz et al. (2017) show that exogenously moving mothers to more family-friendly firms would shrink the gender gap in wages and income. Coudin et al. (2018) show that sorting of workers inter firms explains more of the gender wage gap than bargaining in France, and Bruns (2018) shows high-wage firms disproportionately employ men in Germany.

of their base period earnings, up to a maximum weekly benefit amount.[7]

The DI program was established in 1946 to provide short-term benefits to California workers who experience a loss of wages when they are unable to work due to a non-work-related illness or injury.[8] In 1978, the federal Pregnancy Discrimination Act required that states with DI programs start covering pregnancy as a disability. Birth mothers in California are eligible for four weeks of DI benefits in the period prior to their expected due date, and six weeks of benefits to recover from a vaginal, uncomplicated childbirth (benefits can be extended by two weeks if the delivery is by Cesarean section, or longer if there are other complications). The maximum length of a DI claim for any reason is 52 weeks, though the average claim duration is around 16 weeks. Pregnancy/childbirth-related claims account for approximately one quarter of all DI claims. There is a seven-day non-payable waiting period that must be served for all claims, which reduces the moral hazard problem associated with many sick leave programs. Claimants must also have a physician certify the disability. Workers are only eligible for benefits if they are losing income during their absence, but firms can "top off" DI benefits through employer-provided paid sick leave or other forms of paid time off up to the equivalent of the worker's full salary.

In July 2004, California introduced its PFL program for new parents and caregivers. Eligible workers can take up to six weeks of partially paid leave to bond with a newborn or newly adopted child or to care for a seriously ill family member. The PFL program is structured in the same way as DI, with identical earnings eligibility requirements and wage replacement rate schedules. Both men and women can use the six weeks of PFL, while birth mothers can separately claim both DI and PFL for a total of 16 to 18 weeks of

[7] As of January 1, 2018, the wage replacement rate has increased to 60-70 percent. The 2018 weekly maximum benefit is $1,216.

[8] Work-related injuries are covered under the Worker's Compensation Insurance program, which is separate from DI.

partially paid maternity leave. Between 2004 and 2013, about 90 percent of PFL claims were for bonding with a new child; the remainder were for caregiving purposes. Roughly 74 percent of PFL claims were filed by women, although the gender gap has narrowed over time.

Paid leaves under DI and PFL are not directly job protected, although 12 weeks of job protection is available if the job absence simultaneously qualifies under the federal Family and Medical Leave Act (FMLA) or California's Family Rights Act (CFRA).[9] The lack of job protection may be a significant barrier to DI and PFL take-up for some workers. Other workers may choose not to use available benefits due to career concerns, or because they are unable to afford to take time off with only partial wage replacement.

The firm environment can also play a critical role in determining whether or not employees choose to take leave. Many workers—especially those who are low-income—only hear about government social insurance programs through their employers, if at all (Winston et al., 2017). A survey of a random sample of California registered voters shows that in 2014, a decade after PFL went into effect, only 36 percent of respondents were aware of the program (DiCamillo and Field, 2015). Thus, employers can potentially increase take-up through simply providing their workers with information on the government benefits available to them.

Whether or not California employers have an incentive to encourage eligible workers to use DI and PFL benefits is ambiguous. On one hand, the program provides partially paid leave to workers at no direct cost to firms. Employers do not have to pay workers during the absence, nor do they pay the taxes that finance the program. Thus, SDI allows firms to offer workers the opportunity to take partially paid leave for family or

[9]The FMLA was enacted in 1993 and provides 12 weeks of unpaid job protected family and medical leave to qualifying workers. To be eligible for the FMLA, workers must have worked at least 1,250 hours in the preceding year for an employer with at least 50 employees (within a 75 mile radius of the employment location). The CFRA is nearly identical to the FMLA in its provisions and eligibility criteria.

medical reasons in a relatively cheap way.

On the other hand, worker absences may be costly for firms in other ways. Even if firms do not pay workers for time spent absent from work, productivity may be lower when regular employees are gone, or employers may have to hire temporary replacements. If these costs are high enough, firms may actively discourage workers from utilizing the benefits to which they are entitled. While workers at large firms are legally protected under the FMLA and CFRA during absences of up to 12 weeks, employers may discourage take-up in other ways. For example, they may create a culture where leave-takers are passed over for future promotions, experience slower wage growth, or are assigned less desirable tasks upon their return to work.

4.3 Data

We merge data from two administrative data sets available to us through an agreement with the California Employment Development Department (EDD). The first data set is the universe of DI and PFL claims from fiscal year 2004 to 2013. For each claim, we have information on the type of claim (DI, bonding with a new child, or caring for an ill family member), the claim filed and claim effective dates, the total benefit amount received, the authorized weekly benefit amount, the employee's date of birth and gender, and a unique employee identifier. For women with a PFL bonding claim, we also have an indicator for whether there is an associated DI claim for that birth.

The second data set consists of individual-level quarterly earnings data over 2000-2014 for the universe of employees working for an employer that reports to the EDD tax branch.[10] In addition to the employee identifier (which we use to link to the claims data), it includes earnings in each quarter and in each job, a unique employer identifier

[10] Employers that employ one or more employees and pay wages in excess of $100 in a calendar quarter are required to report to the EDD according to California law.

associated with those earnings, and the North American Industry Classification System (NAICS) industry code associated with the employer. As with most administrative earnings data sets, demographic characteristics about the workers are unavailable. We know worker age and gender only for those individuals who ever file a DI or PFL claim in this period.

4.3.1 Key variables

Because we are interested in the role of firms in social insurance benefit take-up, we collapse the individual-level data to an employer-level panel. For each employer, we calculate average employment and total earnings in each fiscal year (July-June).[11] We then use the claims data to measure the total number of claims taken within a firm in each year.[12] Since eligibility for DI and PFL benefits is determined using base period earnings and not current employment, we link each claim to the individual's employer in the quarter immediately preceding the start of the claim. Therefore, we are attributing the leave to the firm at which the individual worked at the point when he or she most likely decided to make the claim.[13]

We also calculate the number of claims separately by type and gender. Our key dependent variables are: the total number of claims of any type by gender of the claimant, the number of DI claims by gender, the number of bonding claims by gender, and the

[11] We conduct the analysis using fiscal years because PFL became available on July 1, 2004. Our analysis includes fiscal years 2004-2013 (and uses data on claims from July 1, 2004 to June 30, 2014). We have information on DI claims since 2000, and results including these earlier years are very similar. However, in order to be able to better compare the results across different types of claims, the main analysis is limited to the years in which both programs are available.

[12] As mentioned in Section 4.2, birth mothers are eligible to take both DI and PFL for a total of 16 weeks of leave, and this is recorded as two separate claims in the data. From the perspective of both the firm and the mother, this is often taken as a single, continuous period of leave. To avoid double counting leaves taken by these women, we treat associated DI and PFL claims as a single event in the total count of claims.

[13] Some individuals do not have reported earnings in the quarter preceding the claim. For these individuals, we use the employer from two quarters before the claim. This constitutes 3.3 percent of the sample.

number of caring claims by gender. If firms care only about the total number of worker absences and do not differentiate between leaves taken for different reasons, then counting the total number of claims within a firm is reasonable. But we also separate out claims by type because firms may have different attitudes toward leaves related to childcare, family member care, and own health issues, and the effect of the firm premium may differ as well.[14] We separate claims by gender because the overall take-up rates are quite different, and firms may treat male and female employees differently in terms of norms regarding work absences.

To study leave duration and post-leave employment outcomes, we calculate the average leave duration within the firm (conditional on the firm having at least one claim), the share of the firm's claimants that return to work in the firm or in any job within five quarters following the start of the claim, and the average change in log real earnings of claimants between the quarter preceding the leave and the fifth quarter following the start of the claim.[15]

4.3.2 Sample restrictions

We make several restrictions on our analysis sample. First, we exclude firms whose average employment over 2004-2013 is less than 5 employees. We do so because self-employed workers (including independent contractors), individuals who are employers in sole proprietorships or partnerships, and individuals in family employment are not required to participate in the SDI program, and thus are not automatically eligible for benefits. Additionally, the probability of having a claim in any given year is close to

[14]Although women who make associated DI and bonding claims are only counted once in the total claims measure, they are counted as having both a DI and a bonding claim in the counts by claim type. Therefore, the total number of claims is not equal to the sum of the other three measures.

[15]We use five quarters because the maximum length of a DI claim is 52 weeks. Doing so ensures that none of the firm's claimants are still on leave for the relevant claim.

zero for very small firms.[16] Second, because some public sector employees and domestic workers are not covered by SDI, we exclude firms in the three industries least likely to be subject to SDI coverage: elementary and secondary schools, public administration, and private households.

Third, since our main variables of interest are constructed by summing counts over quarterly data, we exclude the 3.8 percent of firm-year observations where the firm is not observed in all four quarters of a given fiscal year. In practice, this restriction implies that we often exclude the year that a firm enters or exits the market. This exclusion is also important because former employees of firms that shut down may be more likely to make a DI or PFL claim as a way to effectively extend unemployment insurance benefits. As we seek to understand how the firm premium affects the likelihood that its current workers make claims, the behavior of workers following a firm closure is not of primary interest in this paper. Finally, as described below, the sample is limited to firms for which we can estimate a firm fixed effect. This restriction effectively excludes firms that are not connected by worker mobility in the sample period (see Section 4 for more detail).

4.3.3 Summary statistics

Our main analysis sample includes 2,709,253 firm-year observations. Table 4.1 shows summary statistics for our main variables of interest. The first row shows the average firm claim rate by claim type. Because overall take-up rates differ substantially by gender, the first four columns show female claims, and the next four columns show male claims. When calculating rates, the denominator is total firm size in the year, as we do not observe the gender of non-claimants in the data. The female overall and DI claim rates are significantly higher than the male claim rates. Even accounting for the fact that only

[16] This restriction drops 68 percent of employer-year observations, but only 7.5 percent of workers. Results are qualitatively and quantitatively similar when only single-person firms are excluded, as shown in Appendix Table B.3.

women can file a DI pregnancy-related claim, women are still more likely than men to make a DI claim. This pattern is true for bonding and caring claims as well.

The remaining rows of Table 4.1 show the mean claim rate by firm size groups, select large industries, and terciles of the firm fixed effect distribution used to estimate firm quality (as described below). Larger and higher fixed effect firms both have higher claim rates, previewing the regression results to come. There is also substantial variation in the firm-level claim rates across industries. Firms in low-skill industries such as retail trade and accommodation and food services have relatively low claim rates. Firms in the healthcare and construction industries both have high female claim rates of about 6.2 percent (for any claim), despite a dramatic difference in the gender composition across the two industries. For context, only 9 percent of California construction workers between 2004 and 2013 were female, compared with 75 percent of workers in the healthcare industry. The age distribution of workers is less dispersed across industries. Between 40 and 56 percent of workers in each industry are of childbearing age (age 20-39), and 27-50 percent are age 40-59. Firms in accommodation and food services have the smallest share of workers above age 40, while health care and manufacturing firms have the largest share. The vast majority–92 percent–of bonding claims are made by workers age 20-39, while workers who make caring claims are somewhat older on average. Women of childbearing age are more likely to make a DI claim than older women, but the opposite is true for men. If 25 percent of all DI claims are for childbirth as estimated by Andrew Chang & Co, LLC (2015), then the non-childbearing related DI claim rates are approximately 36 percent lower for younger compared to older women. This is very similar to the percentage difference in DI claim rates for older and younger men.

Although we do not observe the gender or age composition of employment at the firm level, we do know the demographic characteristics of California workers during this period at a more aggregate level. There are approximately 6.3 female claims per 100 female workers in California, compared to 2.7 male claims per 100 male workers. Female-specific claim rates are again higher than male claim rates for all types of claims. Gender-specific claim rates vary across industries, with health care having the highest any claim rate for both men and women. Importantly, while the levels differ, the pattern of the gender-specific claim rates across industries are similar for men and women. This suggests that the differences in firm-level claim rates in Table 4.1 are not driven by differences in worker composition across different types of firms. Appendix Table B.1 shows these gender-specific claim rates for workers in California.

4.4 Empirical Strategy

Our empirical strategy is comprised of two main steps. First, we estimate firm-specific earnings premiums, following the methodology originally proposed by Abowd, Kramarz, and Margolis (1999) and subsequently used by a growing literature on the role of firms in explaining earnings variance (Abowd et al., 2003; Card et al., 2013, 2016a; Macis and Schivardi, 2016; Lavetti and Schmutte, 2016). The idea is to characterize the natural log of earnings as a function of additive worker and firm fixed effects. The model is identified by job switchers, and predicts that the average earnings change of individuals who move from a low to a high fixed effect firm will be opposite of the average earnings change of individuals who move from a high to a low fixed effect firm.

Specifically, we use our quarterly earnings data from 2000 to 2014 to estimate:

$$E_{ijq} = \alpha_i + \phi_{j(i,q)} + \gamma_q + \varepsilon_{ijq} \tag{4.1}$$

where E_{ijq} is the log quarterly earnings of worker i with primary employer j in quarter

q.[17] The variable α_i is an individual fixed effect, which captures any time-invariant characteristics of the worker that are rewarded equally at all firms. The firm fixed effect, $\phi_{j(i,q)}$, represents the earnings premium that firm j pays to all workers relative to a randomly chosen reference firm.[18] We also flexibly control for aggregate time trends in earnings through quarter fixed effects, γ_q, and ε_{ijq} is an error term.

To reduce the computational burden, equation (4.1) is estimated using every third quarter of data from the first quarter of 2000 through the fourth quarter of 2014.[19] Because we are estimating both worker and firm fixed effects, $\phi_{j(i,q)}$ is identified only within a "connected set" of employers. A group of workers and employers is connected if the group includes all workers who ever worked for any employer in the group and all employers at which any worker in the group was ever employed. We restrict the analysis to the largest connected set, which includes 97.8 percent of firms and 99 percent of workers in the sample of movers (workers observed at more than one firm over time) in California during this period.[20]

A central identifying assumption for estimating unbiased firm fixed effects is that mobility across firms is unrelated to unobserved determinants of earnings changes among

[17] Because some individuals have earnings from multiple employers in the same quarter and we do not observe hours worked, we link workers to the firm at which they have the highest earnings in that quarter. The variable E therefore measures firm-specific earnings in an individual's highest earning job. Appendix Table B.2 shows summary statistics for the AKM model.

[18] Ideally, we would control for total worker experience, but we do not observe employment history prior to 2000. We have also estimated a specification that controls for the worker's cumulative quarters of experience since the first quarter of 2000. The adjusted R^2 of equation (4.1) only increases by about 1 percentage point when this measure is included. Fixed effects generated with the inclusion of this experience measure produce results very similar to our main results, as shown in Appendix Table B.4.

[19] The estimation approach mirrors the Card et al. (2013) algorithm by extracting the sample of workers who changed firms, finding the largest connected set and estimating the fixed effects using numerical methods. We modify Matlab code available on Patrick Kline's website: `http://eml.berkeley.edu/ /~pkline/papers/code_CHK.zip` (retrieved 12/27/2017). We use the full period of earnings data to estimate the fixed effects in order to maximize the number of observations per firm. We have also estimated fixed effects using only data from every quarter 2000-2004. Results are similar and are shown in Appendix Table B.5.

[20] Although the connected set consists of almost all firms and workers within the sample of movers, not all workers change employers between 2000 and 2014. The connected set includes 90.4 percent of all firms and 60.6 percent of all workers in California during this period.

workers. This assumption would be violated if, for instance, workers who were becoming more productive were systematically moving to only certain types of firms. Additionally, model (4.1) assumes additive separability in the firm and worker fixed effects.

As evidence of the plausibility of these assumptions, we follow Card et al. (2013) and Card et al. (2018) and plot mean log earnings for workers in six and three quarters before, the quarter of, and three quarters after a job switch in Figure 4.2. We categorize workers into groups based on the mean earnings quartile of other workers in the old and new firms. Specifically, we classify the earnings quartile of the old job based on mean coworker earnings in the last year at that job, and the earnings quartile of the new job based on mean coworker earnings in the first year at the new job. Job changers are then assigned to one of 16 cells based on the quartiles of the old and new firms. For ease of exposition, Figure 4.2 only shows the earnings trajectories for workers in the eight cells that start at a firm either in the lowest or highest quartile.

The figure shows that, as expected, workers who start in the lowest and highest quartile firms have different initial earnings levels. However, among workers who start out in a firm in the bottom coworker earnings quartile, moving to a firm with higher coworker earnings raises own earnings. Analogously, among those who start in a firm in the top coworker earnings quartile, a move to a lower quartile firm leads to lower own earnings. Those who move to a firm in the same quartile experience very little change in earnings on average. There is no evidence of any transitory change in earnings in the year before or after a move, which, as Card et al. (2013) point out, suggests that the time-varying residual is uncorrelated with mobility. Further, the symmetry of the gains for those who move from the first quartile to a higher quartile and those who move down from the top quartile suggests that a simple additive model of worker and firm fixed effects is reasonable.

The estimated firm fixed effects, $\hat{\phi}_j$, can then be used to evaluate the relationship between the firm earnings premium and paid leave benefit take-up. We first standardize the firm fixed effects, and then estimate the effect of the firm's earnings premium on the number of DI or PFL claims in a firm-year using a Poisson model:

$$Claims_{jnt} = \beta\hat{\phi}_j + \delta ln(size)_{jt} + \psi PctFemale_{nt} + \theta_n + \eta_t + \epsilon_{jnt} \tag{4.2}$$

where $Claims_{jnt}$ is the number of claims in firm j in industry n and fiscal year t. The variable $ln(size)$ represents a firm's average quarterly employment over the fiscal year, $PctFemale$ is the percentage of female employees in the industry-year, and θ_n and η_t are industry and fiscal year fixed effects, respectively.[21] The coefficient of interest, β, captures the effect of a one standard deviation increase in the firm earnings premium on the annual number of claims within the firm. To account for both the over-dispersion in the data and the fact that $\hat{\phi}_j$ is a generated regressor, standard errors are bootstrapped 200 times.[22]

In order to interpret β as the causal effect of the firm earnings premium on the number of claims, the estimated firm fixed effect cannot be correlated with any other unobservable determinants of claims. One particular concern in this context is that we do not know what proportion of the firm's workforce is eligible to file a claim in any given year. While we assume that all of the firm's employees pay into the SDI system, not all workers will have a child and be eligible to make a bonding claim. Similarly, even if all workers are eligible to potentially receive DI benefits, they need to experience a non-work-related illness or injury in order to actually file a successful claim. We are

[21]Data on the percent of female employees in an industry-year in California comes from the 2004-2013 American Community Survey.

[22]If the left-hand side variable is over-dispersed, as is the case here, the Poisson model will still produce a consistent estimate of β. The variance matrix can be consistently estimated using robust standard errors, and bootstrapping produces standard errors that are asymptotically equivalent to the robust standard errors (Cameron and Trivedi, 2013). Bootstrapping in this setting is extremely computationally intensive, but we have estimated the main results using 400 bootstraps and standard errors are almost identical.

therefore assuming that, conditional on firm size, industry, and year, the firm earnings premium is uncorrelated with other demographic characteristics of the firm that would affect the number of claims.

While this assumption is untestable in our data, we show that the effects of the firm premium are robust across type of claim and observable firm characteristics. Moreover, prior research suggests that the types of workers who are most likely to be eligible to take paid leave—e.g., women, who are more likely than men to need leave for childbirth, bonding with a new child, or elder care—are over-represented in low-premium rather than high-premium firms (Card et al., 2016a). Thus, if anything, an unobserved correlation between firm demographics and the firm-specific premium would bias us toward finding a negative association between the firm premium and the leave-taking claim rate, which is the opposite of what we show below. To further address concerns about sorting, we also aggregate the data to the industry level and estimate regressions with and without industry-level controls in Section 4.5.3. This industry-level analysis suggests that our main results are unlikely to be driven by sorting of workers into firms.

Lastly, we test for effects on a large number of outcomes. This creates a multiple inference problem because the probability of making at least one Type I error due to sampling variability is increasing in the number of estimates. We use the Bonferroni method to adjust the p-values to account for the multiple testing problem. This method controls the Family Wise Error Rate (FWER), which is the probability of rejecting at least one true null hypothesis. The Bonferroni correction multiplies each p-value by M, the total number of tests performed on a particular independent variable that are reported in all regular and appendix tables. This ensures that the overall Type I error rate is maintained when performing all M independent hypothesis tests. For example, for an estimated coefficient to be significant at the 1 percent level, we would need a p-value, p, such that $p * M \leq 0.01$. The downside of the method is that it suffers from poor power. As the number of hypotheses increases, the probability of Type II errors (failing to reject the null when there is an effect) also increases. However, because of the size of our data set, the estimated effects are quite precise and this loss of power is less of an issue than in other settings.

4.5 Results

4.5.1 Firm-specific premiums and leave-taking rates

Table 4.2 shows the effect of the firm earnings premium on the number of DI and PFL claims made by employees of the firm in a given fiscal year. The reported coefficients from the Poisson model are incidence rate ratios, obtained by calculating the exponential of the Poisson regression coefficients. Standard errors are similarly transformed. The first column shows that a one standard deviation increase in the firm premium is associated with a 56.9 percent increase in the firm's overall claim rate for any type of claim for both men and women. This effect is estimated with high precision, and the 95 percent confident interval allows us to rule out effects smaller than 54.2 percent.

The remaining columns of Table 4.2 show the effects on the number of claims by gender and claim type. The results present a remarkably consistent story. Higher premium firms have higher claim rates regardless of the type of claim or the gender of the claimant. The percentage effects are somewhat larger for male claims than female claims, and for PFL claims compared to DI claims. These results are not driven by sample restrictions or choices involving the estimation of the firm fixed effects. Appendix Tables B.3-B.5 show the results are robust to including very small firms with 2-4 employees, including observed worker experience in the estimation of the AKM fixed effects, and estimating the fixed effects using only data from 2000 to 2004 (prior to the start of the main estimation sample).[23]

Table 4.3 presents analogous results to Table 4.2, separated into claims made by younger and older workers. The first panel shows the effect of the firm premium on the number of claims among workers ages 20-39. These workers are of childbearing age, and make 92 percent of bonding claims in the estimation sample. About 50 percent of DI claims and 33 percent of caring claims are made by individuals in this age group as well. The second panel shows the effect on the number of claims to workers ages 40-59. These older workers make 40 percent of DI claims and 56 percent of caring claims, but only about 6 percent of bonding claims. While the underlying incidence rates of claims differ across these age groups, the estimated incidence rate ratios of the effect of working for a higher premium firm are similar to the overall results in Table 4.2 for both younger and older workers.

In order to explore if these effects are driven by certain firm characteristics, Table 4.4 shows the effects of the firm premium on the number of claims by firm size and industry. We present results for six firm size groups and the six largest industries, and estimate separate regressions for each group. The results suggest that the effects presented above are not driven by any one particular group. Although the effect of the firm premium is generally increasing in firm size, the effect sizes are economically and statistically significant for even the smallest firms. Interestingly, we do not find substantial differences in the effect of the firm premium on firms with just above versus just below 50 employees. This firm size cutoff is relevant because of eligibility for job protection under the FMLA

[23] We have also estimated a specification that includes a measure of firm skill level as an additional control. We measure average skill by taking the average of the individual fixed effects (estimated in equation 4.1) of the firm's employees over the entire sample period. The estimated effects of the firm premium on the number of claims are very similar with this added control. This again suggests that the sorting of workers into firms is not driving the results. This measure of firm skill level is not included in our main specification because we can only estimate individual fixed effects for movers in the connected set, and so it does not capture the average skill of all workers in the firm. However, these results are available upon request.

and CFRA.[24] This pattern indicates that extending access to job protection may not be enough to reduce the gaps in leave take-up across different types of firms.

There is more variation in the importance of the firm premium across industries. Table 4.4 shows the effects on female claims are largest for firms in the construction sector, while the effects on male claims are largest in accommodation and food services. In general, the effects are consistently positive across industries. The one exception is that the effect of the firm premium on female claims is actually negative for manufacturing firms. Manufacturing firms with a one standard deviation higher fixed effect have 51 percent fewer female claims overall, and the firm premium has no significant effect on the number of male overall or DI claims.[25] There is also no significant effect of the firm premium on DI claims among male workers in the professional, scientific, and technical services industry, but the effects for male PFL claims and all types of female claims are positive and significant. Overall, while there is variation in the effect sizes across industries, there is no clear correlation between the firm premium and industry skill or other industry characteristics.

The results presented so far show that high premium firms have higher leave-taking rates, and these results are consistently significant across claim type, and the gender and age of claimant. The results are also not driven by any particular industry or firm size group. The robustness of the effects of the firm earnings premium on claim rates suggests that the relationship is unlikely to be solely driven by sorting into certain types of firms by workers who need leave. Instead, the similarity of our findings across worker and firm characteristics is more consistent with the interpretation that firm-specific culture—which is associated with the earnings premium—is an important predictor of paid leave

[24] Our measure of firm size is averaged over time, and therefore not a perfect proxy for FMLA/CFRA eligibility. Additionally, FMLA/CFRA eligibility requires the employer to employ 50 or more employees within 75 miles of the work site, whereas we observe total firm size and not establishment size or location.

[25] Incidence rate ratios below 1 indicate a relatively lower likelihood of an event.

use.

However, one may still be concerned that the results are driven by only the highest skilled workers within the firm. If high-premium firms are more supportive of only their top workers taking leave, but are less inclined to support the low-earning workers, then the role of firms in reducing inequality in leave take-up may be less important than it appears. To examine this possibility, we estimate the effect of the firm premium on claims in each quartile of the *firm-specific* earnings distribution in Table 4.5. We find that the firm premium has the strongest effects on the number of claims made by workers in the lower half of the within-firm earnings distribution. In fact, the effects are monotonically *decreasing* in the within-firm earnings quartile. A one standard deviation increase in the firm premium leads to more than a 100 percent increase in the claim rate for all types of claims among workers in the bottom quartile. But the effects of the firm premium on the number of claims in the top quartile are much smaller. For female overall and DI claims, the estimates are actually significantly negative, although relatively small.

As high-ranking employees are the most likely to have access to employer-provided leave benefits and/or flexible schedules, firms appear to play a bigger role in determining public social insurance take-up among workers toward the bottom of the earnings distribution. The results in Table 4.5 imply that high-premium employers are relatively more supportive of their low-earnings workers taking paid leave through DI or PFL compared to lower-premium employers, but the role of the firm premium is less important for relatively high-earning workers within a firm. Therefore, high-premium employers may contribute to reducing disparities in leave use across high- and low-skill individuals.

4.5.2 Firm-specific premiums, leave duration, and post-leave outcomes

The results so far present clear evidence that higher-premium firms have higher paid leave claim rates. However, conditional on having at least one employee who files a claim, firms with higher earnings premiums have shorter average claim durations. Table 4.6 shows that a one standard deviation increase in the firm premium is associated with female claimants taking 1.02 fewer weeks of leave on average. Because average duration is not a count variable, the regression results in this table are estimated using OLS, so the coefficient can be interpreted as the effect of a one standard deviation change in the firm fixed effect on average leave duration in weeks. The effect on DI claim duration is similar for men and women, but the effect on bonding claim duration is more than twice as large for women than for men. This is largely driven by gender differences in mean leave duration. Because birth mothers can also take DI, the firm-level mean bonding leave duration is 14 weeks compared to 3.8 weeks for men. In percentage terms, the effect is about twice as large for male bonding claims. The effect on the duration of caring claims is very similar across claimant gender, and the mean claim lengths are similar as well at 4.3 and 4.0 weeks for women and men, respectively.

There are at least two reasons why higher premium firms may have shorter average leave durations. First, the results on the number of claims suggest that high-premium firms may nudge marginal employees into taking leave, and these marginal claimants may need such leave for shorter amounts of time. Second, these effects are also consistent with the idea that workers may limit the amount of leave they take in order to reduce the risk of separating from a job with a high earnings premium. Not all workers have access to job protection, and even if they do, they may be concerned about the negative career consequences of spending time away from work (Stearns, 2018; Thomas, 2016). While it is not possible to distinguish between these explanations completely, the latter suggests that high fixed effect firms should not only have higher claim rates, but also a higher rate of return to the same firm following a period of leave. While marginal claimants may be more likely to return to work than other claimants, there is less reason to think that, conditional on making a claim, they would be more likely to return to the same firm.

The first row in Table 4.7 shows the effect of the firm earnings premium on the number of claims where the worker returns to employment at any firm within five quarters, with employment defined as having strictly positive earnings in a quarter. These regressions are again estimated with a Poisson model, and we additionally control for the log of the total number of claims within the firm, regardless of whether the claimants return to work. The first column shows that a one standard deviation increase in the firm earnings premium increases the likelihood that a worker who makes a claim returns to employment within five quarters. The effects are similar for female DI and bonding claims as well as male DI claims, but much smaller for male PFL claims. This pattern makes sense, as the firm-level average rate of return to work following a male PFL claim is 96 percent. The average rates of return to employment following a DI or female bonding claim are lower, at around 84 percent for women and 78 percent for male DI claimants.

To evaluate whether high-premium firms have higher employee retention following periods of leave, the second row of Table 4.7 shows the effect on the number of claims where the worker returns to the same firm within five quarters. These results strongly suggest that better firms have much higher retention rates among social insurance claimants. Conditional on the number of claims, a one standard deviation increase in the firm premium increases the probability that female claimants return to the firm by 21 percent and the probability that male claimants return by 24 percent. Though the magnitudes are larger, the pattern across columns is very similar to the effects on returning to any employment, with similar point estimates for male and female DI claims and female bonding claims, but smaller percentage effects for caring and male bonding claims. This is consistent with the idea that workers at high-premium firms want to protect their jobs. It is also consistent, however, with high-premium firms offering more supportive work environments that promote employee retention.

How do these effects of the firm earnings premium on the return to work translate into effects on future earnings? Table 4.8 shows the effects of the firm premium on the average change in log earnings of leave claimants between the quarter prior to the start of the claim and five quarters after the claim, separately by whether the claimants are employed at the same firm or a different firm. This sample is limited to firms that experience at least one claim where the worker is employed at the same firm or a different firm, respectively, in the fifth quarter following the claim. The regressions control for the total number of claims within the firm, regardless of whether or not the workers return to work. The results in the top panel show that for claimants who return to work at the same firm, the firm premium is associated with slightly higher earnings growth. This is consistent with the idea that firms that encourage leave-taking are also less likely to penalize workers who take extended absences. It also may be the case that firms with higher earnings premiums have higher earnings growth in general. On the other hand, workers who file claims in firms with higher premiums and then change employers experience substantially lower subsequent earnings growth compared to those who start out at lower fixed effect firms. These effects are large. A one standard deviation increase in the firm premium is associated with a 32-35 percent drop in earnings for movers who make DI claims and a 19-28 percent drop in earnings for those who make bonding claims.[26] These effects are likely driven by several factors. First, because movers who start at a high premium firm

[26]Table 4.7 shows there is selection into returning to the firm as a function of the firm premium. We have therefore estimated the overall effect of the change in earnings for all claimants who are employed five quarters following the claim and find negative effects. We have also estimated effects for those who return to the same firm but move by the fifth quarter and for those who never returned to the pre-claim employer. These results are available upon request.

are mechanically more likely to move to a firm with a lower premium than are movers who start at a low premium firm (consistent with Figure 4.2), we should expect a negative relationship if the firm premium is a significant determinant of earnings. Second, there might be a direct effect of the employment gap on future earnings that differs among individuals who start at higher versus lower premium firms. Finally, it is important to note that we do not observe work hours and cannot distinguish between changes in wages and changes in employment on the intensive margin. It is possible that workers at high-premium firms leave if these employers are less willing to accommodate part-time work or more flexible schedules. But this explanation seems unlikely given that workers at high-premium firms are actually more likely to return to the firm following a claim.

4.5.3 Alternative Measures

Although the results presented above consistently show that use of public social insurance is positively correlated with the firm earnings premium, it is not possible to identify what characteristics of high premium firms encourage public leave take-up. In particular, one concern is that firms that pay relatively well are compensating for providing less desirable working conditions on other margins. Other work has argued that employee transitions between firms can be used as alternate, revealed preference, measures of firm desirability or quality (Sorkin, 2018; Bagger and Lentz, 2018). In Table 4.9, we show the effect of two additional measures of firm desirability on the number of social insurance claims made within the firm. Panel A shows the effect of a one standard deviation increase in the firm retention rate, measured as the average share of employees who remain employed by the firm from one quarter to the next. These results are qualitatively similar to the effects of the firm earnings premium: both higher premium firms and those with a higher retention rate are more likely to have workers who file claims. Panel B shows the effect of a one standard deviation increase in the poaching index, which is the average share of workers hired in a given quarter who are "poached" from another firm as opposed to coming from non-employment. Bagger and Lentz (2018) argue that the poaching index is an unbiased estimate of the firm's rank in the distribution of firm productivity. Again, firms with a higher poaching index have higher claim rates, although the relationship is generally weaker than it is for the firm premium or firm retention rate. Finally, Panel C shows the effect of the firm earnings premium on the number of claims, controlling for the standardized retention rate and poaching index. Even controlling for these alternate measures of firm quality, the firm premium still has a large and statistically significant effect on the number of social insurance claims.

Finally, one alternate explanation for these results is that the type of people who work at high premium firms are different in ways that are also correlated with program take-up. To address concerns about unobservable sorting of workers into firms, we redo the main analysis at the industry level in Table 4.10. To do this, we calculate the average firm premium in four-digit industries that can be identified in both the EDD data and the ACS.[27] We then regress the number of claims on the industry-aggregated firm premium. There is less concern with worker sorting as a function of desired social insurance use at the industry level, and Sorkin (2018) shows that about 55 percent of the variance in the firm pay premium is between four-digit industries. The first panel of Table 4.10 shows that the results are qualitatively similar when using this industry-level measure of the firm premium. This industry analysis additionally allows us to test for the importance of selection on observables by controlling for other industry-level observable characteristics that may be correlated with firm quality and the likelihood of leave take-up. In the

[27] We use the INDNAICS Specific Variable Codes in the ACS to define industries. While most industries are aggregated at the four digit level, some large industries can be identified at the five or six digit level, and some small industries are aggregated to the two or three digit level. We exclude industries with fewer than 500 ACS observations from 2004-2013.

second panel, we add controls for observable gender-specific industry-level characteristics including the share of workers who have employer-provided health insurance, are foreign-born, are above age 40, are an under-represented minority, and have a four-year college degree, the usual hours worked per week, and the average transportation time to work. The results are very similar when these controls are included, corroborating the idea that selection on observables is relatively unimportant in this setting and that the results are unlikely to be entirely driven by sorting of workers into firms.

4.6 Conclusion

The firm-specific earnings premium is an important predictor of both current and future earnings, and also plays a meaningful role in determining social insurance benefit take-up. In this paper, we first estimate the firm earnings premium using administrative earnings data from California, and then show that higher firm premiums are associated with substantially higher DI and PFL claim rates. This finding is robust across the type of claim, gender and age of the claimant, and other firm characteristics, suggesting that the results cannot be driven by the sorting of workers into firms.

Our findings are important for several reasons. First, the results suggest that firm-specific factors drive disparities in the use of public social insurance. Firms appear to influence inequality in leave-taking, even when benefits are—at least on paper—universally available to workers. As leave-taking is positively correlated with health, employment, and cognitive outcomes of both workers and their families, our findings suggest that firms may contribute not only to wage dispersion, but also to health- and family-related dimensions of inequality in America.

Second, firm-specific attributes appear to be more important in determining social insurance take-up than are changes to specific policy levers. A back of the envelope calculation suggests that DI and PFL take-up would be substantially higher if all employers cultivated a leave-taking culture more similar to that of firms at the top of the firm premium distribution. In contrast, prior work shows that changes to the wage replacement rate or benefit duration have much smaller effects on leave take-up.

Third, short-term leave benefits constitute an increasingly important part of the U.S. social safety net. In 2017, California's DI and PFL programs were the largest source of earnings replacement in the state, paying out a total of over $5.6 billion in benefits. This amount exceeded the $5.3 billion in Unemployment Insurance payments, indicating the extent to which workers value access to short-term paid leave. Our results highlight the important role that firms can play in determining the scale of these programs, which are currently particularly policy relevant as proposals for paid family and medical leave gain substantial momentum at both the state and federal levels.

Although the firm earnings premium is strongly associated with leave-taking claims, we cannot infer the specific aspects of firm behavior or culture that encourage program take-up. Prior work suggests that employers are an important source of information about the existence of these policies and that peer effects within firms play a significant role in determining use (Dahl et al., 2014). It seems likely that these mechanisms are both at play in the California setting as well. Higher premium firms may promote leave-taking for own illness or family care as part of attempts to create a positive and productive workplace culture. If workers can take leave without facing negative career consequences, their peers may be more likely to choose to do so as well. We find that claimants experience earnings losses on average following a period of leave even if they return to the same job, but this is not the case for workers who return to high premium firms. This finding is consistent with the idea that high premium firms are more supportive of their workers taking leave.

One important caveat to these results is that it is not possible to definitively determine whether an increase in DI or PFL take-up is socially optimal. Although these programs serve as an important form of social insurance, they are subject to moral hazard problems. While more research is needed to estimate the welfare gains associated with increased take-up, the consistency of our results across types of claims and types of workers suggests that take-up in lower-premium firms is below the individually-optimal level. In this case, understanding which characteristics of firms promote social insurance take-up is key to extending this form of the social safety net.

Figure 4.1: Mean Earnings of Job Changers Classified by Quartile of Mean Earnings of Coworkers at Origin and Destination Firm

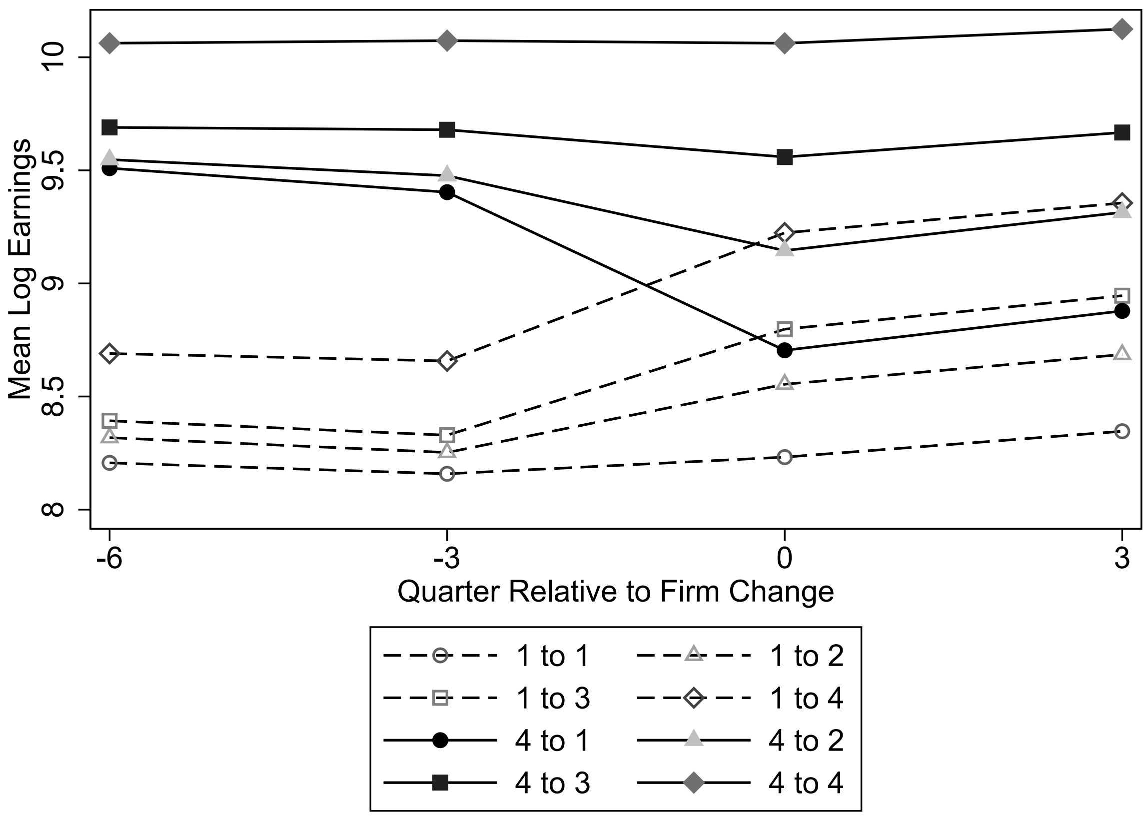


Figure 4.2: *
Figure shows mean log earnings of job changers, classified by quartile of coworker earnings at the origin and destination firm. For ease of interpretation, only workers who start in the top or bottom quartile of the coworker earnings distribution are shown.

Table 4.1: Claim Rates by Firm Characteristics

	Female Claims				**Male Claims**				**Observations**
	Any Claim	DI	Bonding	Caring	Any Claim	DI	Bonding	Caring	
Mean Claim Rate	0.045	0.044	0.014	0.001	0.018	0.016	0.002	0.000	2,709,253
Mean Claim Rate by:									
Firm Size									
Small	0.042	0.041	0.013	0.001	0.016	0.014	0.002	0.000	2,005,409
Medium	0.050	0.048	0.015	0.001	0.023	0.020	0.003	0.000	529,236
Large	0.065	0.062	0.019	0.002	0.029	0.024	0.005	0.001	174,608
Industry									
Construction	0.062	0.060	0.018	0.001	0.027	0.024	0.003	0.000	264,072
Manufacturing	0.046	0.045	0.011	0.001	0.025	0.023	0.002	0.000	238,861
Retail Trade	0.034	0.033	0.010	0.000	0.021	0.019	0.002	0.000	270,993
Professional Services	0.052	0.050	0.020	0.001	0.012	0.009	0.003	0.000	290,219
Health Care	0.062	0.061	0.019	0.001	0.015	0.012	0.003	0.000	335,933
Accommodation	0.030	0.030	0.009	0.000	0.010	0.009	0.001	0.000	314,595
Firm Fixed Effect Terciles									
Low	0.034	0.034	0.010	0.000	0.012	0.011	0.001	0.000	903,081
Middle	0.048	0.046	0.014	0.001	0.021	0.018	0.002	0.000	903,082
High	0.053	0.051	0.018	0.001	0.022	0.018	0.003	0.000	903,090

Notes: Table shows mean claim rates at the firm-year level from fiscal year 2004-2013. The measure of firm size used in calculating rates is time-varying. Small firms have 5-24 workers, medium firms have 25-99 workers, and large firms have more than 100 workers. For this classification, firm size is averaged over all years the firm appears in the sample and is constant over time. Industries shown are the six largest industries in California. Professional Services is Professional, Scientific, and Technical Services, and Accommodation is Accommodation and Food Services. Firm fixed effects are estimated using the AKM methodology as explained in Section 4.4 and divided into terciles.

Table 4.2: Effect of Firm Premium on Number of Leave-Taking Claims

	All	**Female Claims**				**Male Claims**			
	Any Claim	Any Claim	DI	Bonding	Caring	Any Claim	DI	Bonding	Caring
Firm Premium	1.569*	1.447*	1.427*	1.512*	2.076*	1.797*	1.660*	2.628*	2.589*
	(0.014)	(0.013)	(0.013)	(0.013)	(0.041)	(0.026)	(0.021)	(0.051)	(0.058)
Mean Number of Claims	2.218	1.407	1.352	0.369	0.037	0.811	0.671	0.123	0.017

Notes: Table shows the effect of the firm premium on the number of DI or PFL claims within the firm in a given year. All columns include 2,709,253 observations. The effects are estimated using a Poisson regression and the estimates and standard errors have been exponentiated such that the coefficients shown here are incidence rate ratios. Sample includes fiscal years 2004-2013. Controls include firm size, year fixed effects, industry fixed effects, and the share of women in the industry-year. Standard errors (in parentheses) are bootstrapped 200 times. To account for multiple hypothesis testing, p-values have been corrected using the Bonferroni correction. * $p<0.01$.

Table 4.3: Effect of Firm Premium on Number of Leave-Taking Claims by Age of Claimant

	Female Claims				Male Claims			
	Any Claim	DI	Bonding	Caring	Any Claim	DI	Bonding	Caring
Claims at Age 20-39								
Firm Premium	1.427*	1.410*	1.526*	2.127*	1.868*	1.585*	2.633*	2.524*
	(0.012)	(0.012)	(0.013)	(0.048)	(0.030)	(0.025)	(0.052)	(0.070)
Mean Number of Claims	0.804	0.778	0.345	0.012	0.348	0.235	0.106	0.007
Claims at Age 40-59								
Firm Premium	1.586*	1.561*	2.078*	2.132*	1.809*	1.757*	2.660*	2.700*
	(0.022)	(0.022)	(0.031)	(0.047)	(0.027)	(0.026)	(0.065)	(0.077)
Mean Number of Claims	0.484	0.459	0.016	0.022	0.367	0.342	0.016	0.009

Notes: Table shows the effect of the firm premium on the number of DI or PFL claims by age of the claimant within the firm in a given year. All regressions include 2,709,253 observations. The effects are estimated using a Poisson regression and the estimates and standard errors have been exponentiated such that the coefficients shown here are incidence rate ratios. Sample includes fiscal years 2004-2013. Controls include firm size, year fixed effects, industry fixed effects, and the share of women in the industry-year. Standard errors (in parentheses) are bootstrapped 200 times. To account for multiple hypothesis testing, p-values have been corrected using the Bonferroni correction. * $p<0.01$.

Table 4.4: Effect of Firm Premium on Number of Leave-Taking Claims by Firm Size and Industry

	Female Claims				**Male Claims**				**Observations**
	Any Claim	DI	Bonding	Caring	Any Claim	DI	Bonding	Caring	
Firm Premium									
Firm Size 5-9	1.172*	1.165*	1.410*	1.178	1.363*	1.302*	2.004*	1.655*	1,115,617
	(0.006)	(0.006)	(0.012)	(0.048)	(0.011)	(0.011)	(0.045)	(0.136)	
Firm Size 10-24	1.217*	1.208*	1.486*	1.305*	1.597*	1.504*	2.650*	2.166*	889,792
	(0.005)	(0.005)	(0.012)	(0.046)	(0.010)	(0.009)	(0.054)	(0.125)	
Firm Size 25-49	1.292*	1.276*	1.581*	1.699*	1.793*	1.648*	3.515*	2.574*	347,930
	(0.008)	(0.007)	(0.015)	(0.064)	(0.014)	(0.013)	(0.071)	(0.148)	
Firm Size 50-99	1.328*	1.309*	1.591*	1.804*	2.076*	1.888*	3.869*	3.373*	181,306
	(0.008)	(0.008)	(0.016)	(0.061)	(0.017)	(0.016)	(0.082)	(0.178)	
Firm Size 100-499	1.413*	1.385*	1.600*	2.181*	2.145*	1.925*	3.709*	3.516*	143,719
	(0.009)	(0.009)	(0.014)	(0.049)	(0.018)	(0.018)	(0.051)	(0.107)	
Firm Size 500+	1.566*	1.547*	1.545*	2.233*	1.701*	1.578*	2.346*	2.353*	30,889
	(0.022)	(0.022)	(0.023)	(0.061)	(0.043)	(0.041)	(0.072)	(0.086)	
Construction	2.302*	2.255*	2.831*	3.377*	2.737*	2.560*	4.804*	4.022*	264,072
	(0.068)	(0.067)	(0.114)	(0.438)	(0.039)	(0.037)	(0.128)	(0.260)	
Manufacturing	0.485*	0.476*	0.754*	0.672*	1.066	0.973	1.951*	1.427*	238,861
	(0.018)	(0.018)	(0.022)	(0.044)	(0.033)	(0.030)	(0.089)	(0.089)	
Retail Trade	1.443*	1.430*	1.203*	2.086*	2.933*	2.812*	3.513*	3.622*	270,993
	(0.034)	(0.033)	(0.038)	(0.101)	(0.084)	(0.081)	(0.187)	(0.156)	
Professional Services	1.246*	1.222*	1.644*	1.624*	1.284*	1.071	2.316*	2.002*	290,219
	(0.026)	(0.025)	(0.028)	(0.064)	(0.025)	(0.022)	(0.059)	(0.078)	
Health Care	1.753*	1.730*	1.906*	2.466*	2.032*	1.776*	3.256*	3.198*	335,933
	(0.020)	(0.019)	(0.022)	(0.080)	(0.040)	(0.035)	(0.091)	(0.188)	
Accommodation	1.400*	1.371*	1.057	8.242*	3.648*	3.291*	7.222*	17.105*	314,595
	(0.026)	(0.025)	(0.016)	(0.619)	(0.089)	(0.079)	(0.320)	(1.536)	

Notes: Table shows the effect of the firm premium on the number of DI or PFL claims within the firm in a given year, subsampled by firm size and industry groups. Firm size categories are based on employment averaged over all years in the data, and are constant over time. Professional Services is Professional, Scientific, and Technical Services, and Accommodation is Accommodation and Food Services. The effects are estimated using a Poisson regression and the estimates and standard errors have been exponentiated such that the coefficients shown here are incidence rate ratios. Sample includes fiscal years 2004-2013. Controls include firm size, year fixed effects, industry fixed effects, and the share of women in the industry-year. Standard errors (in parentheses) are bootstrapped 200 times. To account for multiple hypothesis testing, p-values have been corrected using the Bonferroni correction. * $p<0.01$.

Table 4.5: Effect of Firm Premium on Number of Leave-Taking Claims by Within-Firm Earnings Quartile of Claimant

	Female Claims				**Male Claims**			
	Any Claim	DI	Bonding	Caring	Any Claim	DI	Bonding	Caring
Quartile 1								
Firm Premium	2.454*	2.420*	2.470*	4.049*	2.631*	2.427*	4.892*	4.800*
	(0.048)	(0.046)	(0.030)	(0.209)	(0.037)	(0.033)	(0.131)	(0.189)
Mean Number of Claims	0.284	0.274	0.069	0.006	0.137	0.120	0.014	0.002
Quartile 2								
Firm Premium	1.751*	1.722*	1.795*	2.908*	2.358*	2.136*	3.890*	4.052*
	(0.019)	(0.019)	(0.018)	(0.080)	(0.032)	(0.027)	(0.091)	(0.121)
Mean Number of Claims	0.411	0.396	0.105	0.011	0.207	0.172	0.030	0.004
Quartile 3								
Firm Premium	1.278*	1.259*	1.445*	1.778*	1.876*	1.695*	2.954*	2.783*
	(0.013)	(0.012)	(0.016)	(0.034)	(0.029)	(0.026)	(0.071)	(0.080)
Mean Number of Claims	0.398	0.383	0.107	0.011	0.238	0.193	0.039	0.005
Quartile 4								
Firm Premium	0.938*	0.923*	1.005	1.226*	1.236*	1.152*	1.654*	1.581*
	(0.009)	(0.009)	(0.011)	(0.029)	(0.018)	(0.017)	(0.031)	(0.041)
Mean Number of Claims	0.314	0.300	0.087	0.009	0.230	0.185	0.039	0.005

Notes: Table shows the effect of the firm premium on the number of DI or PFL claims within the firm in a given year, subsampled by the within-firm earnings quartile of claimants. Quartile 1 is the lowest 25 percent of earners within the firm and quartile 4 is the highest. The effect in each quartile is estimated from a separate regression. All regressions include 2,709,253 observations. The effects are estimated using a Poisson regression and the estimates and standard errors have been exponentiated such that the coefficients shown here are incidence rate ratios. Sample includes fiscal years 2004-2013. Controls include firm size, year fixed effects, industry fixed effects, and the share of women in the industry-year. Standard errors (in parentheses) are bootstrapped 200 times. To account for multiple hypothesis testing, p-values have been corrected using the Bonferroni correction. * $p<0.01$.

Table 4.6: Effect of Firm Premium on Mean Claim Duration (Weeks)

	Female Claims				**Male Claims**			
	Any Claim	DI	Bonding	Caring	Any Claim	DI	Bonding	Caring
Firm Premium	-1.016*	-1.255*	-0.983*	-0.382*	-2.048*	-1.410*	-0.475*	-0.313*
	(0.023)	(0.024)	(0.020)	(0.021)	(0.036)	(0.035)	(0.013)	(0.031)
Observations	717,453	705,925	337,788	42,980	524,970	481,444	117,671	25,490
Mean Claim Duration	11.643	10.181	13.991	4.258	11.383	12.514	3.769	4.007

Notes: Table shows the effect of the firm premium on the mean claim duration (measured in weeks) within a firm-year, conditional on having at least one claim. The effects are estimated using an OLS regression. Sample includes fiscal years 2004-2013. Controls include firm size, year fixed effects, industry fixed effects, and the share of women in the industry-year. Standard errors (in parentheses) are bootstrapped 200 times. To account for multiple hypothesis testing, p-values have been corrected using the Bonferroni correction. * $p<0.01$.

Table 4.7: Effect of Firm Premium on Number of Leave-Taking Claimants Who Return to Work

	Female Claims				**Male Claims**			
	Any Claim	DI	Bonding	Caring	Any Claim	DI	Bonding	Caring
Return to Employment								
Firm Premium	1.089*	1.089*	1.096*	1.044*	1.092*	1.096*	1.020*	1.031*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.001)	(0.003)
Mean Number of Claims Returning to Employment	4.794	4.675	2.674	2.273	3.659	3.222	2.754	1.795
Return to Firm								
Firm Premium	1.213*	1.216*	1.255*	1.101*	1.239*	1.254*	1.107*	1.090*
	(0.005)	(0.005)	(0.004)	(0.005)	(0.008)	(0.008)	(0.005)	(0.007)
Mean Number of Claims Returning to Firm	4.249	4.135	2.370	2.137	3.123	2.705	2.499	1.682
Observations (all rows)	717,453	705,925	337,788	42,980	524,970	481,444	117,671	25,490

Notes: Table shows the effect of the firm premium on the number of claims made by workers who return to employment at any firm within five quarters of the start of the claim (first row) and the effect of the firm premium on the number of claims made by workers who return to work at the same firm within five quarters of the start of the claim (second row). The effects are estimated using a Poisson regression and the estimates and standard errors have been exponentiated such that the coefficients shown here are incidence rate ratios. Sample includes fiscal years 2004-2013. Regressions control for the log number of claims made within the firm-year, unconditional on returning to work. Additional controls include firm size, year fixed effects, industry fixed effects, and the share of women in the industry-year. Standard errors (in parentheses) are bootstrapped 200 times. To account for multiple hypothesis testing, p-values have been corrected using the Bonferroni correction. * $p<0.01$.

Table 4.8: Effect of Firm Premium on the Average Change in Earnings of Claimants

	Female Claims				Male Claims			
	Any Claim	DI	Bonding	Caring	Any Claim	DI	Bonding	Caring
Employed At Same Firm								
Firm Premium	0.054*	0.052*	0.083*	0.017	0.051*	0.038*	0.058*	0.031*
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.006)	(0.007)
Observations	406,298	400,631	187,464	36,686	264,654	241,810	71,420	21,358
Mean Change in Log Earnings	-0.070	-0.069	-0.071	-0.016	-0.066	-0.073	0.009	-0.014
Employed At Different Firm								
Firm Premium	-0.331*	-0.323*	-0.275*	-0.031*	-0.352*	-0.346*	-0.185*	-0.013
	(0.006)	(0.006)	(0.010)	(0.007)	(0.007)	(0.007)	(0.017)	(0.014)
Observations	246,327	244,117	100,685	27,803	176,361	165,063	38,880	16,227
Mean Change in Log Earnings	-0.223	-0.218	-0.186	-0.054	-0.209	-0.205	-0.095	-0.042

Notes: Table shows the effect of the firm premium on the mean change in log real earnings of claimants between the quarter prior to the start of the claim and five quarters after the claim, conditional on the firm having at least one claimant who returns to employment. The top panel shows the effect for those who are employed at the same firm in the fifth quarter after the claim, and the second panel shows the effect for those who are employed at a different firm in the fifth quarter after the claim. The effects are estimated using an OLS regression. Sample includes fiscal years 2004-2013. Regressions control for the log number of claims made within the firm-year, unconditional on returning to work. Additional controls include firm size, year fixed effects, industry fixed effects, and the share of women in the industry-year. Standard errors (in parentheses) are bootstrapped 200 times. To account for multiple hypothesis testing, p-values have been corrected using the Bonferroni correction. * $p<0.01$.

Table 4.9: Effect of Alternate Firm Quality Measures on Number of Leave-Taking Claims

	Female Claims				Male Claims			
	Any Claim	DI	Bonding	Caring	Any Claim	DI	Bonding	Caring
Panel A								
Retention Rate	1.899*	1.854*	1.616*	5.945*	2.021*	1.812*	3.594*	6.370*
	(0.021)	(0.020)	(0.015)	(0.182)	(0.031)	(0.028)	(0.082)	(0.229)
Panel B								
Poaching Index	1.062*	1.054*	1.150*	1.434*	1.403*	1.330*	1.890*	1.609*
	(0.011)	(0.011)	(0.010)	(0.030)	(0.016)	(0.015)	(0.032)	(0.041)
Panel C								
Firm Premium	1.218*	1.207*	1.413*	1.189*	1.564*	1.475*	2.076*	1.616*
	(0.015)	(0.014)	(0.014)	(0.027)	(0.027)	(0.026)	(0.048)	(0.043)
Retention Rate	1.550*	1.528*	1.157*	4.663*	1.334*	1.273*	1.682*	3.580*
	(0.022)	(0.022)	(0.013)	(0.160)	(0.024)	(0.023)	(0.042)	(0.142)
Poaching Index	1.018	1.012	1.078*	1.348*	1.312*	1.245*	1.85*	1.543*
	(0.010)	(0.010)	(0.009)	(0.028)	(0.017)	(0.016)	(0.036)	(0.041)
Mean Number of Claims	1.407	1.353	0.369	0.037	0.811	0.671	0.123	0.017

Notes: Table shows the effect of measures of firm quality on the number of DI or PFL claims within the firm in a given year. Panel A shows the effect of the firm's standardized average quarterly retention rate, and all columns include include 2,709,253 observations. Panel B shows the effect of the firm's standardized average quarterly poaching index, and all columns include 2,709,155 observations. Panel C includes the standardized firm earnings premium, retention rate, and poaching index, and all columns include 2,709,155 observations. The correlation between the firm premium and the retention rate is 0.47, the correlation between the firm premium and the poaching index is 0.21, and the correlation between the retention rate and poaching index is 0.01. The effects are estimated using a Poisson regression and the estimates and standard errors have been exponentiated such that the coefficients shown here are incidence rate ratios. Sample includes fiscal years 2004-2013. Controls include firm size, year fixed effects, industry fixed effects, and the share of women in the industry-year. Standard errors (in parentheses) are bootstrapped 200 times. To account for multiple hypothesis testing, p-values have been corrected using the Bonferroni correction. * $p<0.01$.

Table 4.10: Effect of Industry Average Firm Premium on Number of Leave-Taking Claims

	Female Claims				Male Claims			
	Any Claim	DI	Bonding	Caring	Any Claim	DI	Bonding	Caring
No Industry-Level Controls								
Average Industry Premium	1.973*	1.931*	1.701*	4.308*	1.482*	1.353*	2.235*	2.411*
	(0.097)	(0.093)	(0.050)	(0.451)	(0.072)	(0.067)	(0.143)	(0.228)
With Industry-Level Controls								
Average Industry Premium	1.880*	1.868*	1.990*	2.378*	1.484*	1.402*	1.793*	1.538*
	(0.106)	(0.104)	(0.096)	(0.222)	(0.089)	(0.085)	(0.124)	(0.145)
Mean Number of Claims	1935.832	1861.357	510.254	51.242	1093.192	904.659	165.147	23.385

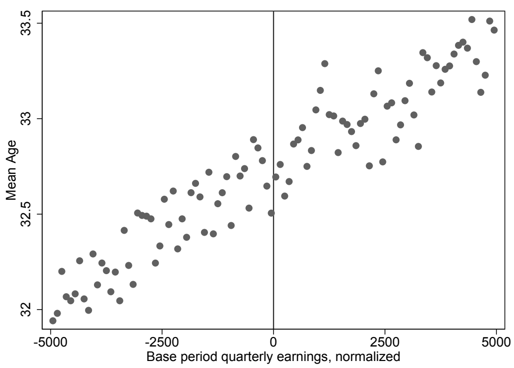
Notes: Table shows the effect of the industry average firm premium on the number of DI or PFL claims within the industry in a given year. All columns include 1,920 observations from 192 industries. The effects are estimated using a Poisson regression and the estimates and standard errors have been exponentiated such that the coefficients shown here are incidence rate ratios. Sample includes fiscal years 2004-2013. All regressions include industry size and year fixed effects as well as the percentage of the industry that is female. The bottom panel additionally includes gender-specific industry-level controls for the share of workers who have employer-provided health insurance, are foreign-born, are above age 40, are an under-represented minority, and have a four-year college degree, the usual hours worked per week, and the average transportation time to work. Standard errors (in parentheses) are bootstrapped 200 times. To account for multiple hypothesis testing, p-values have been corrected using the Bonferroni correction. * $p<0.01$.

Appendix A

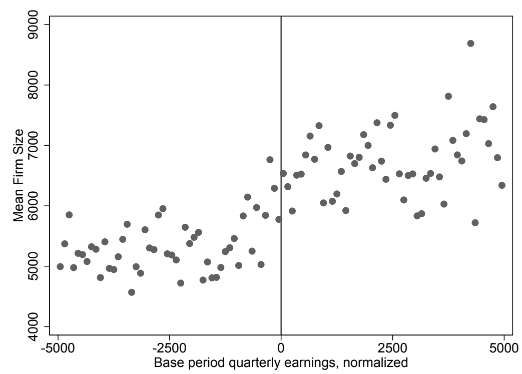
Appendix for The Impact of Paid Family Leave Benefits: Regression Kink Evidence from California Administrative Data

A.1 Appendix Figures and Tables

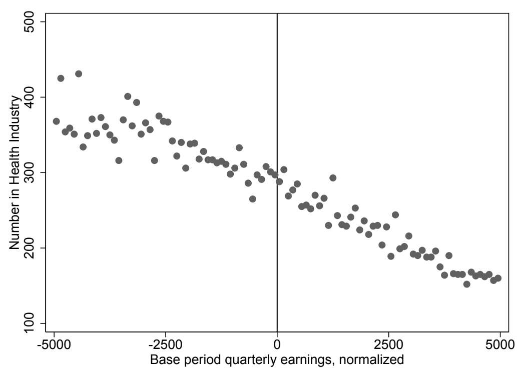
Figure A.1: Covariates Around the Earnings Threshold



(a) Age



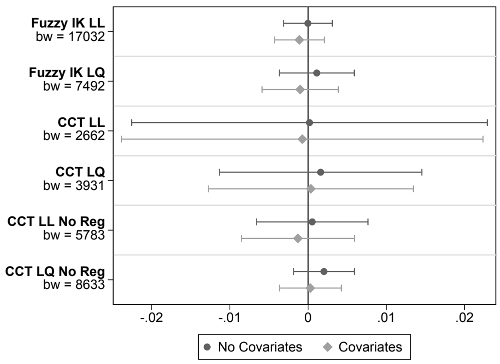
(b) Firm Size



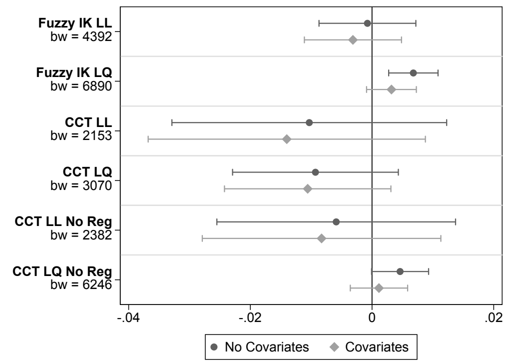
(c) Health Industry

Notes: The $x-$axis plots normalized base period quarterly earnings (relative to the earnings threshold in each year) in bins, using \$100 bins. In sub-figures (a) and (b), the $y-$axis plots the mean of the covariate in each bin. In sub-figure (c), the $y-$axis plots the count of women in the health industry in each bin.

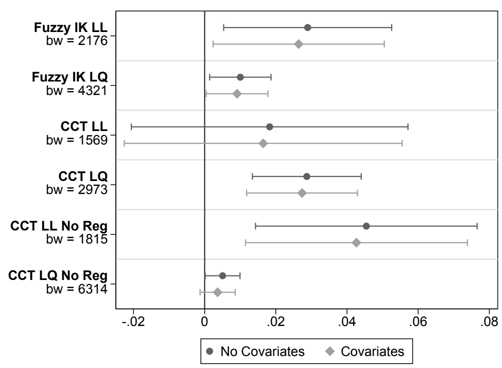
Figure A.2: RK Estimates for Main Outcomes Using Different Specifications, Using Benefit Amount in Levels



(a) Log Leave Duration



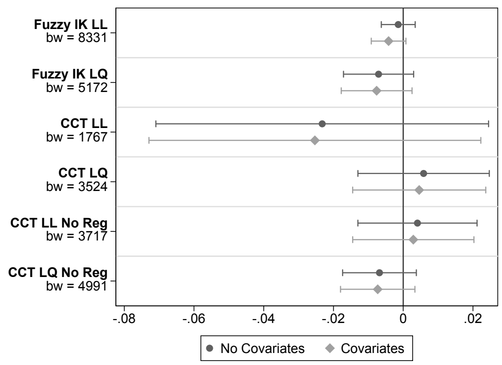
(b) Employed, 2 Qtrs. Post-Claim



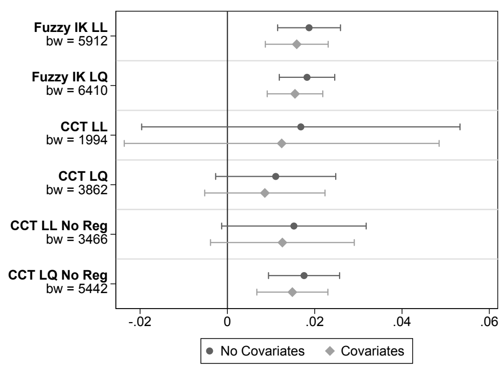
(c) Same Firm (if Employed)

Notes: These figures show the coefficients and 95% confidence intervals (as horizontal bars) from different RK specifications, estimated separately with and without individual-level controls. The coefficients and standard errors are for the effect of a $100 increase in the WBA. See notes under Figure 3.3 for more details about the outcomes. All regressions include year×quarter and week-of-quarter of the claim fixed effects. The specifications with individual controls include the following variables: indicators for employee age categories (20-24, 25-29, 30-34, 35-39, 40-44), dummies for pre-claim employer industry (NAICS industry groups), and dummies for employer size (1-49, 50-99, 100-499, 500+). The specification models are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. The optimal bandwidths from each specification are listed.

Figure A.3: RK Estimates for Main Outcomes Using Different Specifications, Using Benefit Amount in Levels



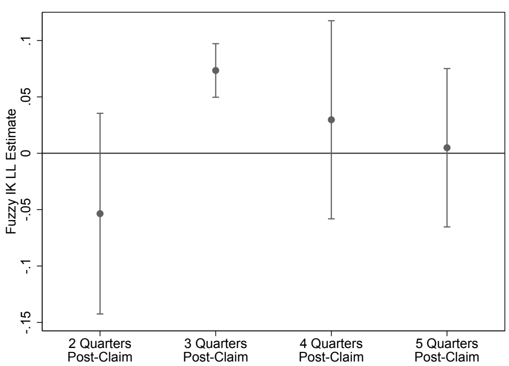
(a) Δ Log Earnings



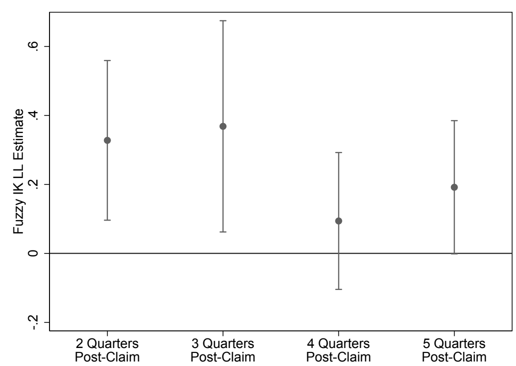
(b) Any Subsequent Bonding Claim

Notes: These figures show the coefficients and 95% confidence intervals (as horizontal bars) from different RK specifications, estimated separately with and without individual-level controls. The coefficients and standard errors are for the effect of a $100 increase in the WBA. See notes under Figure 3.3 for more details about the outcomes. All regressions include year×quarter and week-of-quarter of the claim fixed effects. The specifications with individual controls include the following variables: indicators for employee age categories (20-24, 25-29, 30-34, 35-39, 40-44), dummies for pre-claim employer industry (NAICS industry groups), and dummies for employer size (1-49, 50-99, 100-499, 500+). The specification models are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. The optimal bandwidths from each specification are listed.

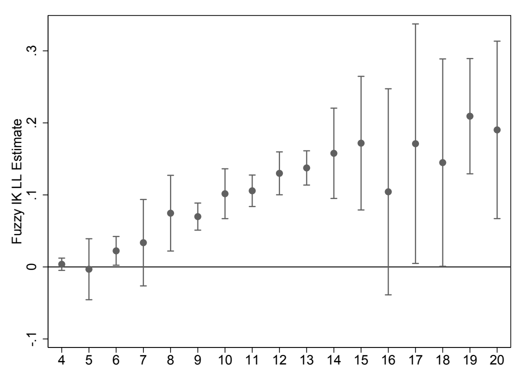
Figure A.4: Timing of Effects on Employment, Return to Firm, and Subsequent Bonding Claims



(a) Employment



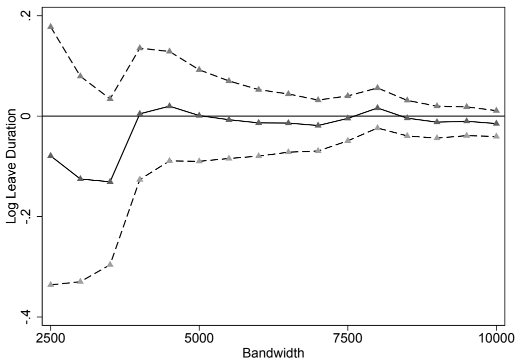
(b) Same Firm (if Employed)



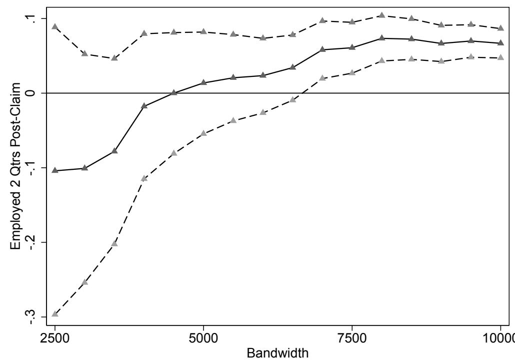
(c) Any Subsequent Bonding Claim (Cumulative)

Notes: These figures show the coefficients and 95% confidence intervals (as vertical bars) from separate regression models that use the fuzzy IK with a local linear polynomial specification. As outcomes, sub-figures (a) and (b) use indicators for employment and employment in the pre-claim firm (conditional on any employment) in quarters 2 through 5 post-claim, as listed on the $x-$axis. Sub-figure (c) uses indicators for any subsequent bonding claim *by* the quarter listed on the $x-$axis. All regressions include year×quarter and week-of-quarter of the claim fixed effects.

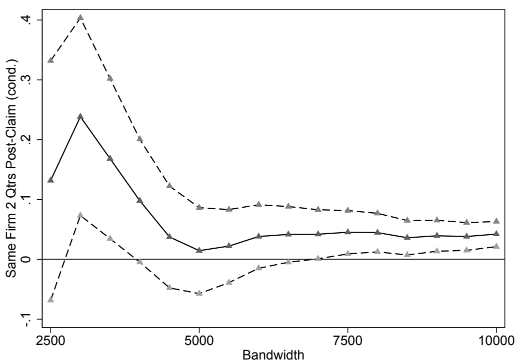
Figure A.5: RK Estimates for Main Outcomes Using Different Bandwidths: 2005-2010 Only



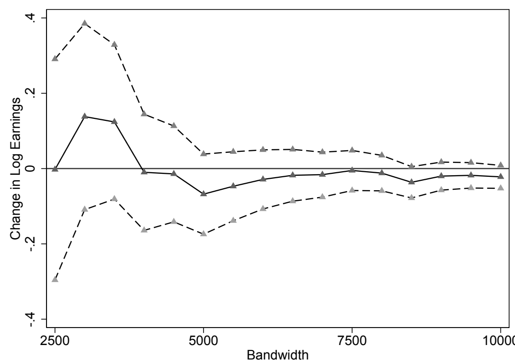
(a) Log Leave Duration



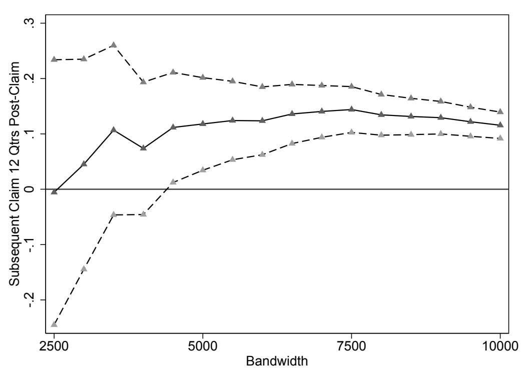
(b) Employed, 2 Qtrs. Post-Claim



(c) Same Firm (if Employed)



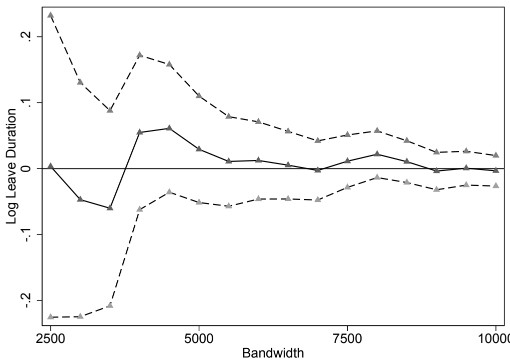
(d) Δ Log Earnings



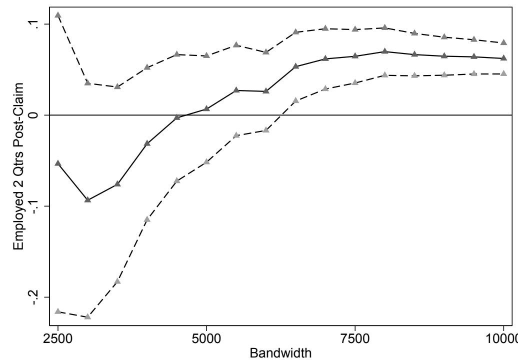
(e) Any Subsequent Bonding Claim

Notes: These figures show the coefficients (as dark gray triangles) and 95 percent confidence intervals (as light gray triangles) from RK specifications that use different bandwidths in increments of \$500 of normalized quarterly base period earnings (denoted on the $x-$axis). The sample is limited to claims made in 2005-2010 only. All regressions include year$\times$quarter and week-of-quarter of the claim fixed effects.

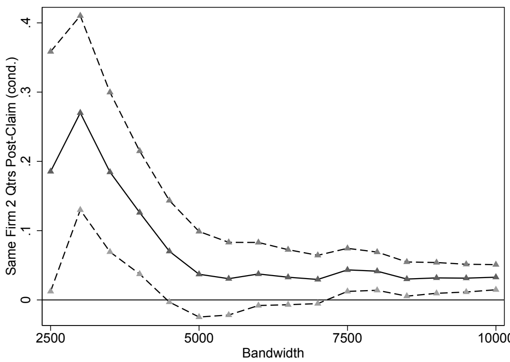
Figure A.6: RK Estimates for Main Outcomes Using Different Bandwidths: Drop Information Industry



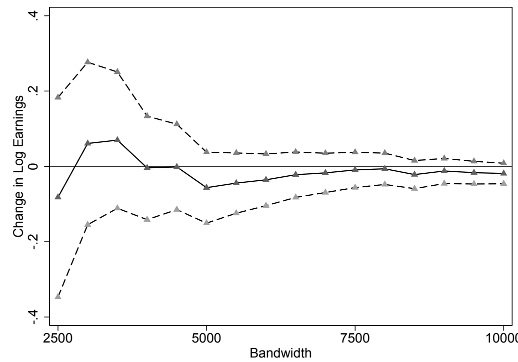
(a) Log Leave Duration



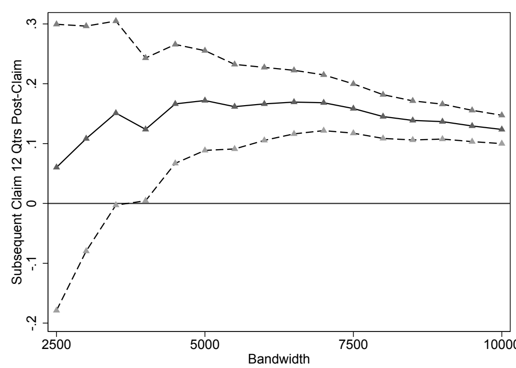
(b) Employed, 2 Qtrs. Post-Claim



(c) Same Firm (if Employed)



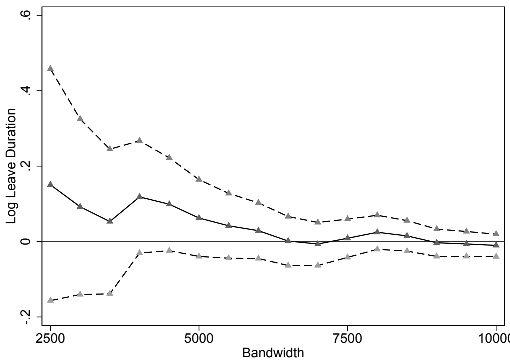
(d) Δ Log Earnings



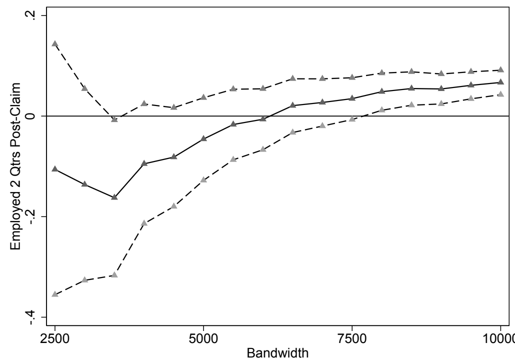
(e) Any Subsequent Bonding Claim

Notes: These figures show the coefficients (as dark gray triangles) and 95 percent confidence intervals (as light gray triangles) from RK specifications that use different bandwidths in increments of \$500 of normalized quarterly base period earnings (denoted on the $x-$axis). We drop women employed in the Information industry (NAICS group 51). All regressions include year×quarter and week-of-quarter of the claim fixed effects.

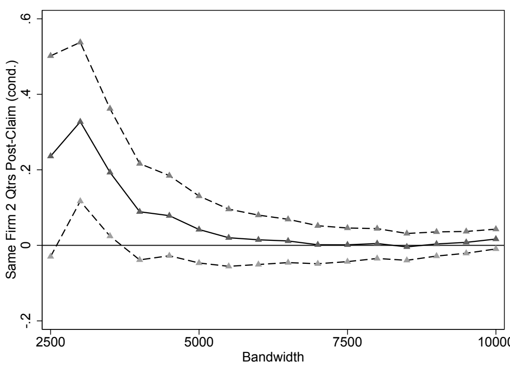
Figure A.7: RK Estimates for Main Outcomes Using Different Bandwidths: Firms with <1,000 Employees Only



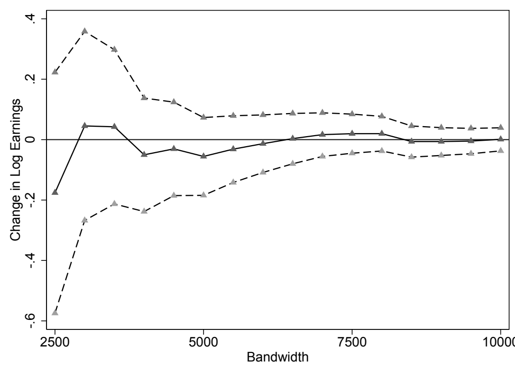
(a) Log Leave Duration



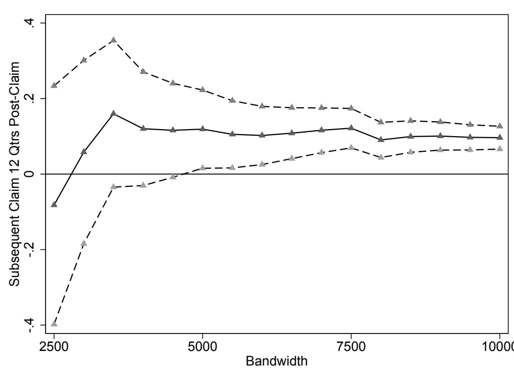
(b) Employed, 2 Qtrs. Post-Claim



(c) Same Firm (if Employed)



(d) Δ Log Earnings



(e) Any Subsequent Bonding Claim

Notes: These figures show the coefficients (as dark gray triangles) and 95 percent confidence intervals (as light gray triangles) from RK specifications that use different bandwidths in increments of $500 of normalized quarterly base period earnings (denoted on the $x-$axis). The sample is limited to claims made by women in firms with fewer than 1,000 employees only. All regressions include year×quarter and week-of-quarter of the claim fixed effects.

Table A.1: Descriptive Statistics in ACS Data

	2500	5000	7500	10000
Mother's age	34.14	33.96	33.78	33.38
	(4.103)	(4.077)	(4.179)	(4.321)
Mother is non-Hispanic white	0.471	0.476	0.466	0.458
	(0.499)	(0.500)	(0.499)	(0.498)
Mother is non-Hispanic black	0.0360	0.0359	0.0418	0.0455
	(0.186)	(0.186)	(0.200)	(0.208)
Mother is Hispanic	0.110	0.121	0.137	0.172
	(0.313)	(0.326)	(0.344)	(0.377)
Mother is married	0.929	0.914	0.902	0.878
	(0.257)	(0.280)	(0.297)	(0.327)
Spousal annual earnings ($2014)	93742.2	90712.1	86742.1	81028.4
	(82422.3)	(83893.3)	(82695.2)	(79378.1)
Observations	931	1,846	2,938	4,171

Notes: This table uses data from the 2005-2014 American Communities Survey (ACS) and presents means and standard deviations (in parentheses) of characteristics of mothers who are comparable to our main analysis sample of female bonding claimants in the EDD data. We limit to mothers of children under age 1 in California and make restrictions similar to those that we make in the EDD data: (1) We only include women who are aged 20-44; (2) We drop women employed in industries in which employees are least likely to be subject to the SDI tax—private household workers, elementary and secondary school teachers, and public administration; (3) We drop women with zero reported earnings in the previous year. We use each woman's prior year earnings to calculate her average quarterly earnings (by dividing by four), and then use that to find her place in the prior year's benefit schedule (and assign her to the appropriate kink point). We report statistics for women with earnings in the bandwidths listed at the top of each column. All statistics are weighted using ACS person weights.

Table A.2: RK Estimates of the Effects of PFL Benefits on Log Leave Duration

	(1) Fuzzy IK LL	(2) Fuzzy IK LQ	(3) CCT LL	(4) CCT LQ	(5) CCT LL, No Reg	(6) CCT LQ, No Reg
A. No Individual Controls						
Log WBA ($2014)	0.0118	0.0153	-0.00322	0.00788	-0.00445	0.0178
	(0.0151)	(0.0192)	(0.106)	(0.0597)	(0.0315)	(0.0151)
First Stage Est $\times 10^5$	-5.850	-4.131	-4.887	-4.661	-5.203	-4.162
First Stage S.E. $\times 10^5$	0.0320	0.159	0.192	0.421	0.0604	0.127
B. With Individual Controls						
Log WBA ($2014)	-0.00152	-0.00172	-0.0117	-0.00354	-0.0204	0.00478
	(0.0156)	(0.0198)	(0.109)	(0.0612)	(0.0323)	(0.0156)
First Stage Est $\times 10^5$	-5.668	-4.104	-4.714	-4.578	-5.060	-4.156
First Stage S.E. $\times 10^5$	0.0311	0.151	0.181	0.400	0.0580	0.121
Main Bandwidth	8690.2	7565.3	2664.4	3923.4	5731.8	8632.5
Pilot Bandwidth	6797.8	6148.1	5351.9	6316.7	7821.4	9381.2
Dep. Var Mean	2.396	2.396	2.394	2.395	2.396	2.396
N	197691	165856	54150	80687	120751	195915

Notes: Each coefficient in each panel and column is from a separate regression, using the natural log of total leave duration as the outcome. The WBA is expressed as the natural log of $2014 dollars. The top panel only includes year×quarter and week-of-quarter of the claim fixed effects and no individual controls, while the bottom panel includes the following controls: indicators for employee age categories (20-24, 25-29, 30-34, 35-39, 40-44), dummies for pre-claim employer industry (NAICS industry groups), and dummies for employer size (1-49, 50-99, 100-499, 500+). The specifications are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. We also report the first stage coefficients and standard errors, the dependent variable means, and the main and pilot bandwidths. The pilot bandwidth is used in the bias estimation part of the main bandwidth selection procedure. Robust standard errors are in parentheses.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table A.3: RK Estimates of the Effects of PFL Benefits on Employment in Quarter 2 Post-Claim

	(1) Fuzzy IK LL	(2) Fuzzy IK LQ	(3) CCT LL	(4) CCT LQ	(5) CCT LL, No Reg	(6) CCT LQ, No Reg
A. No Individual Controls						
Log WBA ($2014)	-0.0536	0.0261	-0.0932	-0.0842	-0.0530	0.0426**
	(0.0454)	(0.0220)	(0.104)	(0.0635)	(0.0901)	(0.0202)
First Stage Est $\times 10^5$	-4.868	-4.361	-4.963	-5.486	-4.950	-4.334
First Stage S.E.$\times 10^5$	0.114	0.229	0.271	0.614	0.237	0.212
B. With Individual Controls						
Log WBA ($2014)	-0.0678	-0.00388	-0.128	-0.0969	-0.0753	0.0129
	(0.0463)	(0.0224)	(0.107)	(0.0645)	(0.0908)	(0.0205)
First Stage Est $\times 10^5$	-4.712	-4.311	-4.787	-5.328	-4.845	-4.303
First Stage S.E. $\times 10^5$	0.108	0.218	0.254	0.585	0.224	0.201
Main Bandwidth	3810.2	5911.8	2153.1	3070.2	2381.5	6246.1
Pilot Bandwidth	5226.5	6462.5	4908.2	4817.7	5182.6	5758.3
Dep. Var Mean	0.876	0.871	0.876	0.876	0.875	0.870
N	74929	119900	41946	59981	46432	127450

Notes: Each coefficient in each panel and column is from a separate regression, using an indicator for employment in quarter 2 post-claim as the outcome. The WBA is expressed as the natural log of $2014 dollars. The top panel only includes year×quarter and week-of-quarter of the claim fixed effects and no individual controls, while the bottom panel includes the following controls: indicators for employee age categories (20-24, 25-29, 30-34, 35-39, 40-44), dummies for pre-claim employer industry (NAICS industry groups), and dummies for employer size (1-49, 50-99, 100-499, 500+). The specifications are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. We also report the first stage coefficients and standard errors, the dependent variable means, and the main and pilot bandwidths. The pilot bandwidth is used in the bias estimation part of the main bandwidth selection procedure. Robust standard errors are in parentheses.
Significance levels: * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Table A.4: RK Estimates of the Effects of PFL Benefits on Employment in Pre-Claim Firm (Conditional on Any Employment) in Quarter 2 Post-Claim

	(1) Fuzzy IK LL	(2) Fuzzy IK LQ	(3) CCT LL	(4) CCT LQ	(5) CCT LL, No Reg	(6) CCT LQ, No Reg
A. No Individual Controls						
Log WBA ($2014)	0.328***	0.125***	0.170	0.262***	0.416***	0.0401*
	(0.118)	(0.0439)	(0.185)	(0.0714)	(0.147)	(0.0209)
First Stage Est $\times 10^5$	-5.021	-4.485	-4.692	-5.600	-4.866	-4.242
First Stage S.E. $\times 10^5$	0.320	0.454	0.450	0.706	0.371	0.228
B. With Individual Controls						
Log WBA ($2014)	0.321***	0.116***	0.155	0.255***	0.394***	0.0284
	(0.122)	(0.0448)	(0.188)	(0.0742)	(0.148)	(0.0214)
First Stage Est $\times 10^5$	-4.827	-4.182	-4.566	-5.470	-4.769	-4.218
First Stage S.E. $\times 10^5$	0.302	0.429	0.427	0.669	0.354	0.216
Main Bandwidth	2041.1	4044.9	1568.7	2972.5	1815.3	6314.2
Pilot Bandwidth	3626.8	6181.7	3390.3	4654.1	3609.2	12454.9
Dep. Var Mean	0.880	0.876	0.883	0.877	0.880	0.875
N	34799	69821	26707	50857	30924	112124

Notes: Each coefficient in each panel and column is from a separate regression, using an indicator for employment in the pre-claim firm in quarter 2 post-claim (conditional on any employment in that quarter) as the outcome. The WBA is expressed as the natural log of $2014 dollars. The top panel only includes year×quarter and week-of-quarter of the claim fixed effects and no individual controls, while the bottom panel includes the following controls: indicators for employee age categories (20-24, 25-29, 30-34, 35-39, 40-44), dummies for pre-claim employer industry (NAICS industry groups), and dummies for employer size (1-49, 50-99, 100-499, 500+). The specifications are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. We also report the first stage coefficients and standard errors, the dependent variable means, and the main and pilot bandwidths. The pilot bandwidth is used in the bias estimation part of the main bandwidth selection procedure. Robust standard errors are in parentheses.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table A.5: RK Estimates of the Effects of PFL Benefits on Change in Log Earnings (Qtrs. 2-5 Post vs. 2-5 Pre-Claim)

	(1) Fuzzy IK LL	(2) Fuzzy IK LQ	(3) CCT LL	(4) CCT LQ	(5) CCT LL, No Reg	(6) CCT LQ, No Reg
A. No Individual Controls						
Log WBA ($2014)	-0.0166	-0.0586	-0.210	0.0464	0.0371	-0.0641
	(0.0184)	(0.0462)	(0.221)	(0.0882)	(0.0792)	(0.0472)
First Stage Est $\times 10^5$	-5.843	-4.265	-4.889	-5.522	-4.733	-4.249
First Stage S.E. $\times 10^5$	0.0392	0.340	0.418	0.587	0.136	0.347
B. With Individual Controls						
Log WBA ($2014)	-0.0398**	-0.0622	-0.230	0.0346	0.0268	-0.0694
	(0.0191)	(0.0469)	(0.222)	(0.0906)	(0.0819)	(0.0480)
First Stage Est $\times 10^5$	-5.641	-3.950	-4.842	-5.129	-4.552	-3.993
First Stage S.E. $\times 10^5$	0.0380	0.321	0.399	0.555	0.129	0.328
Main Bandwidth	8558.8	5056.8	1767.0	3523.6	3717.2	4991.4
Pilot Bandwidth	4575.6	6546.6	3565.5	5874.1	4354.7	6776.6
Dep. Var Mean	-0.103	-0.102	-0.100	-0.103	-0.103	-0.102
N	143938	79307	27210	54633	57685	78234

Notes: Each coefficient in each panel and column is from a separate regression, using the change in log earnings from quarters 2-5 before the claim to quarters 2-5 after the claim. The WBA is expressed as the natural log of $2014 dollars. The top panel only includes year×quarter and week-of-quarter of the claim fixed effects and no individual controls, while the bottom panel includes the following controls: indicators for employee age categories (20-24, 25-29, 30-34, 35-39, 40-44), dummies for pre-claim employer industry (NAICS industry groups), and dummies for employer size (1-49, 50-99, 100-499, 500+). The specifications are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. We also report the first stage coefficients and standard errors, the dependent variable means, and the main and pilot bandwidths. The pilot bandwidth is used in the bias estimation part of the main bandwidth selection procedure. Robust standard errors are in parentheses.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table A.6: RK Estimates of the Effects of PFL Benefits on Any Subsequent Bonding Claim in 12 Quarters Post-Claim

	(1) Fuzzy IK LL	(2) Fuzzy IK LQ	(3) CCT LL	(4) CCT LQ	(5) CCT LL, No Reg	(6) CCT LQ, No Reg
A. No Individual Controls						
Log WBA ($2014)	0.130***	0.162***	0.152	0.0954	0.139*	0.151***
	(0.0152)	(0.0255)	(0.168)	(0.0623)	(0.0773)	(0.0352)
First Stage Est $\times 10^5$	-6.078	-4.305	-5.014	-4.516	-4.768	-4.330
First Stage S.E. $\times 10^5$	0.0368	0.229	0.350	0.523	0.146	0.305
B. With Individual Controls						
Log WBA ($2014)	0.117***	0.141***	0.113	0.0753	0.116	0.129***
	(0.0154)	(0.0259)	(0.167)	(0.0633)	(0.0776)	(0.0355)
First Stage Est $\times 10^5$	-5.895	-4.316	-4.944	-4.454	-4.662	-4.273
First Stage S.E. $\times 10^5$	0.0359	0.217	0.333	0.495	0.139	0.289
Main Bandwidth	8775.0	6555.2	1993.8	3862.1	3466.3	5441.7
Pilot Bandwidth	5919.6	7057.1	4031.7	6134.7	4926.5	7248.6
Dep. Var Mean	0.210	0.221	0.235	0.232	0.232	0.226
N	152885	106065	30620	59889	53582	86093

Notes: Each coefficient in each panel and column is from a separate regression, using an indicator for any subsequent bonding claim in the 12 quarters following the first claim as the outcome. The WBA is expressed as the natural log of $2014 dollars. The top panel only includes year×quarter and week-of-quarter of the claim fixed effects and no individual controls, while the bottom panel includes the following controls: indicators for employee age categories (20-24, 25-29, 30-34, 35-39, 40-44), dummies for pre-claim employer industry (NAICS industry groups), and dummies for employer size (1-49, 50-99, 100-499, 500+). The specifications are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. We also report the first stage coefficients and standard errors, the dependent variable means, and the main and pilot bandwidths. The pilot bandwidth is used in the bias estimation part of the main bandwidth selection procedure. Robust standard errors are in parentheses.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table A.7: Difference-in-Difference Estimates of the Effects of PFL Benefits on Main Outcomes

	(1)	(2)	(3)	(4)	(5)
	Log Duration	Emp. 2 Qtrs Post-Claim	Same Firm (if Emp.)	Δ Log Earn.	Subs. Bond.
A. No Earnings-Bin-Specific Linear Time Trends					
Log WBA ($2014)	0.0243***	-0.0497***	0.188***	0.150***	0.0798***
	(0.00593)	(0.00376)	(0.00632)	(0.00836)	(0.00435)
B. With Earnings-Bin-Specific Linear Time Trends					
Log WBA ($2014)	0.0232***	-0.0495***	0.188***	0.150***	0.0793***
	(0.00594)	(0.00377)	(0.00635)	(0.00838)	(0.00436)
N	240,541	231,308	197,778	178,030	184,979

Notes: Each coefficient in each panel and column is from a separate regression. See notes under Figure 3.3 for more details about the outcomes. All regressions include $1,000 earnings bin fixed effects, as well as year×quarter and week-of-quarter of the claim fixed effects. The specifications in Panel B also include linear trends interacted with earnings bin indicators. Robust standard errors are in parentheses.
Significance levels: * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Appendix B

Appendix for Unequal Use of Social Insurance: The Role of Employers

B.1 Appendix Tables

Table B.1: Claim Rates by Worker Characteristics

	Female Claims				Male Claims			
	Any Claim	DI	Bonding	Caring	Any Claim	DI	Bonding	Caring
Number of Claims	4,032,876	3,879,858	1,061,309	104,248	2,307,892	1,914,805	344,613	48,474
Claim Rate	0.063	0.061	0.017	0.002	0.027	0.023	0.004	0.001
Claim Rate by Age:								
20-39	0.080	0.077	0.034	0.001	0.025	0.017	0.008	0.000
40-59	0.051	0.049	0.002	0.002	0.030	0.028	0.001	0.001
Claim Rate by Industry:								
Construction	0.051	0.049	0.015	0.001	0.026	0.023	0.003	0.000
Manufacturing	0.062	0.059	0.013	0.002	0.033	0.028	0.004	0.001
Retail Trade	0.074	0.071	0.018	0.002	0.036	0.030	0.006	0.001
Professional Services	0.050	0.048	0.019	0.001	0.016	0.011	0.005	0.000
Health Care	0.079	0.076	0.018	0.003	0.037	0.029	0.008	0.001
Accommodation	0.055	0.054	0.016	0.001	0.019	0.016	0.002	0.000

Notes: Table shows mean gender-specific claim rates at the worker-year level from fiscal year 2004-2013. Claims data is merged with data from the American Community Survey 2004-2013 to create gender-specific employment counts by year. Industries shown are the six largest industries in California. Professional Services is Professional, Scientific, and Technical Services, and Accommodation is Accommodation and Food Services. Note that this table is representative of workers, whereas Table 4.1 is representative of firms. This table also includes workers at very small firms of 1-4 workers, which are excluded from our main analysis, because firm size is not available. Workers at firms of 1-4 workers only make up 7.8 percent of the California workforce and 3.6 percent of claims.

Table B.2: AKM Model Summary Statistics

	Full Sample	Movers	Largest Connected Set
Sample Size			
Person-Quarters	300,424,074	227,840,807	227,614,272
Individuals	34,166,334	20,740,162	20,716,651
Firms			2,203,086
Summary Statistics			
Mean Log Earnings	8.868	8.792	8.793
Standard Deviation of Log Earnings	1.278	1.265	1.265
Summary of Parameter Estimates			
Standard Deviation of Firm Effects			0.591
Standard Deviation of Person Effects			0.751
Correlation of Person/Firm Effects			0.226
RMSE of AKM Residual			0.739
Adjusted R^2			0.659
Comparison Match Model			
RMSE of AKM Residual			0.534
Adjusted R^2			0.822
Model Including Potential Experience			
RMSE of AKM Residual			0.731
Adjusted R^2			0.666

Notes: Sample includes every third quarter from the first quarter of 2000 through 2014. There is one observation per person-quarter. If an individual held multiple jobs, the observation is the job from which they had the highest earnings. The comparison match model includes interactions between employers and individuals. The model including potential experience includes the number of past quarters the person is observed in the data.

Table B.3: Effect of Firm Premium on Number of Leave-Taking Claims, Including 2-4 Person Firms

	All	**Female Claims**				**Male Claims**			
	Any Claim	Any Claim	DI	Bonding	Caring	Any Claim	DI	Bonding	Caring
Firm Premium	1.545*	1.429*	1.410*	1.495*	2.001*	1.760*	1.633*	2.381*	2.368*
	(0.01)	(0.012)	(0.012)	(0.011)	(0.031)	(0.023)	(0.022)	(0.042)	(0.046)
Mean Number of Claims	1.366	0.868	0.835	0.229	0.023	0.498	0.412	0.075	0.011

Notes: Table shows the effect of the firm premium on the number of DI or PFL claims within the firm in a given year. All columns include 4,498,541 observations. Sample includes all firms with an average of two or more employees. The effects are estimated using a Poisson regression and the estimates and standard errors have been exponentiated such that the coefficients shown here are incidence rate ratios. Sample includes fiscal years 2004-2013. Standard errors (in parentheses) are bootstrapped 200 times. To account for multiple hypothesis testing, p-values have been corrected using the Bonferroni correction. * $p<0.01$.

Table B.4: Effect of Firm Premium on Number of Leave-Taking Claims, Firm Fixed Effects Estimated Controlling for Experience

	All	**Female Claims**				**Male Claims**			
	Any Claim	Any Claim	DI	Bonding	Caring	Any Claim	DI	Bonding	Caring
Firm Premium	1.594*	1.472*	1.451*	1.534*	2.125*	1.822*	1.681*	2.666*	2.654*
	(0.01)	(0.014)	(0.014)	(0.014)	(0.044)	(0.024)	(0.022)	(0.053)	(0.063)
Mean Number of Claims	2.218	1.407	1.352	0.369	0.037	0.811	0.671	0.123	0.017

Notes: Table shows the effect of the firm premium on the number of DI or PFL claims within the firm in a given year. All columns include 2,709,253 observations. The firm premium fixed effects are estimated while additionally controlling for the worker's experience, measured as the number of past quarters they are observed in the data. The effects are estimated using a Poisson regression and the estimates and standard errors have been exponentiated such that the coefficients shown here are incidence rate ratios. Sample includes fiscal years 2004-2013. Controls include firm size, year fixed effects, industry fixed effects, and the share of women in the industry-year. Standard errors (in parentheses) are bootstrapped 200 times. To account for multiple hypothesis testing, p-values have been corrected using the Bonferroni correction. * $p<0.01$.

Table B.5: Effect of Firm Premium on Number of Leave-Taking Claims, Firm Fixed Effects Estimated Using 2000-2004 Data

	All	**Female Claims**				**Male Claims**			
	Any Claim	Any Claim	DI	Bonding	Caring	Any Claim	DI	Bonding	Caring
Firm Premium	1.454*	1.356*	1.340*	1.389*	1.718*	1.606*	1.530*	1.883*	1.909*
	(0.02)	(0.014)	(0.014)	(0.014)	(0.037)	(0.026)	(0.022)	(0.046)	(0.050)
Mean Number of Claims	2.556	1.620	1.557	0.417	0.044	0.935	0.774	0.140	0.021

Notes: Table shows the effect of the firm premium on the number of DI or PFL claims within the firm in a given year. All columns include 2,137,839 observations. The firm premium fixed effects are estimated using earnings data from every quarter of 2000-2004. The effects are estimated using a Poisson regression and the estimates and standard errors have been exponentiated such that the coefficients shown here are incidence rate ratios. Sample includes fiscal years 2004-2013. Controls include firm size, year fixed effects, industry fixed effects, and the share of women in the industry-year. Standard errors (in parentheses) are bootstrapped 200 times. To account for multiple hypothesis testing, p-values have been corrected using the Bonferroni correction. * $p<0.01$.

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